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PROPERTIES OF MATTER

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PROPERTIES OF MATTER

BY

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PREFACE.

In spite of what I must still consider a tolerably complete statement of my position (§§ 11–15, 108, etc.), some of my critics persist in accusing me of deliberate inconsistency in my treatment of Force. I am represented by them as first telling my readers that there is "no such thing as Force," and then introducing utter confusion by the assertion that "Matter is merely the plaything of Force"! They appear to be unable to perceive that the idea of Force is an *essential* feature of Newton's Laws of Motion:—and that, until we are provided with an efficient substitute for Newton's system, we must retain it in its integrity. To have left these statements altogether unnoticed might have been prejudicial to the book itself, in the eyes of the many weak ones who regard the dicta of a critic (especially when he is anonymous) as necessarily authoritative. To take more than this passing notice of them would be to exaggerate their importance.

Since the last edition of this book was published, the whole of M. Amagat's splendid experimental results have become generally accessible:—and I have made considerable additional use of them, especially in Chaps. IX. and X.

Parts of Chap. VIII. have been considerably modified, with the view of making easier for beginners the excessively important elements of the mathematical theory of deformation.

I am greatly indebted to Sir G. G. Stokes for assistance in my endeavour to form clear conceptions on the vexed question of the (so-called) *internal pressure* of fluids. It was alluded to only in a foot-note to § 280 of the previous edition, but it now forms the subject of § 260 of the text. The difficulties which this question undoubtedly presents seem to be mainly of our own making; another illustration of the pernicious effects of a false system, in which force is regarded as something essentially objective.

The whole work has been carefully revised by Dr C. G. Knott as well as by myself, and the proofs have undergone minute scrutiny from Messrs W. Thomson and A. D. Russell, Neil-Arnott Scholars in my Laboratory.

P. G. TAIT.

COLLEGE, EDINBURGH,
May 1, 1894.

PREFACE TO THE SECOND EDITION.

In the present edition this Treatise has been carefully revised and considerably extended:—special attention having been paid to passages where a difficulty had been found.

For one of the most important additions I am indebted to M. Amagat, who has very kindly enabled me to avail myself of some of his splendid but hitherto unpublished results. These relate to the compression of fluids exposed to enormous pressures; and, when published entire, will form a singularly interesting and practically new branch of the subject.

To some of the scientific critics of the first edition I am indebted for suggestions of real value, and I have endeavoured to profit by them. I must except, however, those which concern my treatment of the subject of *Force.* I have seen so much mischief done by this quasi-personification of a mere sense-impression that, even in an elementary book, I am constrained to protest against it. (See § 15 of the text.) I feel assured that the difficulties which are now everywhere felt as to the great scientific question of the day, the nature of what we call electricity, are in great part due to the way in which our modes of thinking have been, by early training and subsequent habit, encouraged to run in this fatal groove.

To some of my other critics, more aggressive because less scientific, I have been indebted for genuine amusement. Nothing is, however, without its use in this world, though it may occasionally be difficult to discover that use. It would seem, then, that the function of the unscientific critics of a scientific book is (like that of the writers of slipshod English) to furnish examiners with rich material for questions of the well-known kind:—"Point out *all* the errors in the following passage." Nothing is more difficult than the attempt to *make* such passages:—and the results are usually forced and awkward. From the critics I allude to they come in perfection.

There is one additional remark which I must make. The majority of the illustrations in this work (whether given in words or by diagrams) are, when the contrary is not stated, to the best of my knowledge original. I make the remark lest I should be supposed to have taken them from some of the books in which they have been reproduced without acknowledgment of their source. It is flattering to have one's work thus appreciated, but the honour has its little inconveniences.

P. G. TAIT.

College, Edinburgh,
July 1, 1890.

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PROPERTIES OF MATTER.

CHAPTER I.

INTRODUCTORY.

1. We start with certain assumptions or Axioms, which are not of an *à priori* character, but which the observations and experiments of many generations have forced upon us:—

(1) That the physical universe has an objective existence.

(2) That we become cognisant of it solely by the aid of our Senses.

(3) That the indications of the Senses are always imperfect, and often misleading; but

(4) That the patient exercise of Reason enables us to control these indications, and gradually, but surely, to sift truth from falsehood.

2. If, for a moment, we use the word *Thing* to denote, generally, whatever we are constrained to allow has objective existence:—*i.e.* exists altogether independently of our senses and of our reason:—we arrive at the following conclusions:—

A. In the physical universe there are but two classes of things, Matter and Energy.

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but the waste which he laments he looks upon as its *annihilation*. Till within the last fifty years or so the vast majority even of scientific men held precisely the same opinion.

7. The modern doctrine of the *Conservation of Energy*, securely based upon the splendid investigations of Joule and others, completes the justification of our preliminary statement. Energy, like matter, has been experimentally proved to be indestructible and uncreatable by man. It exists, therefore, altogether independently of human senses and human reason, though it is known to man solely by their aid.

One of the most curious passages in history is that which describes the quest of *The Perpetual Motion*. This was simply the attempt to discover a continuous *Source* of fresh mechanical energy. In 1775 the Academy of Sciences declined, for the future, to consider any scheme which professed to furnish work without corresponding and equivalent expenditure. But the race of Perpetual Motionists is by no means even yet extinct. The doctrine of the impossibility of the Perpetual Motion is often valuable in modern physics (see, for instance, § 139 below), as it furnishes simple *ex absurdo* proofs of important fundamental theorems.

The objectivity of energy is virtually admitted in a curious way, by its being advertised for sale. Thus in manufacturing centres, where a mill-owner has a steam-engine too powerful for his requirements, he issues a notice to the effect, "Spare Power to let." But, of course, the common phrase "price of labour" at once acknowledges the objectivity of work.

8. There is, however, a most important point to be noticed. Energy is never found except in association

with matter. Hence we might define matter as the *Vehicle* or *Receptacle of Energy;* and it is already more than probable that energy will ultimately be found, in all its varied forms, to depend upon *Motion* of matter.

This is advanced, for the moment, as a mere introductory statement, instances of which will be discussed even in the present work; but its complete treatment would require the introduction of branches of physics with which we have here nothing to do. One great argument in its favour is, that matter is found to consist of parts which preserve their identity, while energy is manifested to us only in the act of transformation, and (though measurable) cannot be identified. For this is precisely what we should *expect* to find if energy depends invariably on motion of matter.

9. Beside their common characteristic, conservation, and in strange contrast to it, we have their characteristic difference. Matter is simply passive (*inert* is the scientific word); energy is perpetually undergoing transformation. The one is, as it were, the body of the physical universe; the other its life and activity. All terrestrial phenomena, from winds and waves to lightning and thunder, eruptions and earthquakes, are transformations of energy. So are alike the brief flash of a falling star, and the fiery glow from the mighty solar outbursts of incandescent hydrogen.

10. From the *strictly* scientific point of view, the greater part of the present work would be said to deal with energy rather than with matter. In fact, were we to speak of weight as a *property of matter*, in the sense that a stone of itself has weight, or even in the sense that the earth *attracts* the stone, we should go directly in the teeth of Newton's distinct assertion.

For such a statement (because confined to the attracting bodies alone) implies the existence of *Action at a Distance*, a very old but most pernicious heresy, of which much more than traces still exist among certain schools, even of physicists. (See Newton's words on this subject, § 160 below.)

Gravitation, like all other mutual actions between particles of matter, such as give rise to cohesion, elasticity, etc., must, with our present knowledge, be set down to the energy which particles of matter are found to possess when separated. The intervening mechanism by which this is to be accounted for has, as yet, only been guessed at, and none of the guesses have been successful. Clerk-Maxwell's success in explaining electric and magnetic attractions by something analogous to stresses and rotations in the luminiferous ether shows, however, that we need not despair of being able to explain the ultimate mechanism of gravitation.

But there is great convenience in separating, as far as possible, the treatment of Mass, Weight, Cohesion, Elasticity, Viscosity, etc., which we range under the general title, *Properties of Matter*, from that of Heat, Light, Electric Energy, etc., which can all in great measure be studied without express reference to any one special kind of matter—though, as forms of energy, they exist only (§ 8 above) in association with matter. Along with these forms of energy must of course be treated the allied properties of matter, such as specific heat, refractive index, conductivity, etc. Such, therefore, are foreign to the present work. And it must be remarked that, even in popular language, we invariably speak of the hardness of a body, its rigidity, its elasticity, as belonging to it in much the same sense as does its density or its atomic weight—and certainly in a much more intimate sense than does its temperature or its electric potential.

It is, therefore, on the two grounds of custom and convenience that we use the term *Properties of Matter* as the title of this work. The error involved is not by any means so monstrous as that which all agree to perpetuate by the use of the term *Centrifugal Force.*

11. The word *Force* must often, were it only for brevity's sake, be used in the present work. As it does not denote either matter or energy, it is not a term for anything objective (§ 2, A). The idea it is meant to express is suggested to us by the "muscular sense," just as the ideas of brightness, noise, smell, or pain are suggested by other senses:—though they do not correspond directly to anything which exists outside us.

It is exceedingly difficult to realize fully the fact that noise is a mere subjective impression, even when reason has convinced us that outside the drum of the ear there is nothing to correspond to it except a periodic compression and dilatation of the air.

Still more difficult is it to realize that outside us *all is dark;* and that the objective cause of even the most gorgeous of optical phenomena is an excessively rapid quivering motion of the ethereal jelly which extends through all space.

We need not, therefore, be surprised at the tenacity with which the great majority, even of scientific men, still cling to the notion of force as something objective.

But if it were objective, what an absolutely astounding difficulty would have to be faced by one who tries to explain the nature of hydrostatic pressure; and who finds that by the touch of a finger on a little piston he can produce a pressure of (say) a pound weight on every square inch of the surface of a vessel, however large, if full of water, and the same amount on every square inch of surface of every object immersed in it, even if that object consisted of hundreds of square miles of sheets of tinfoil far enough apart to let the water penetrate between them. All this, moreover, is found to disappear the moment he lifts his finger!

When we communicate energy to a body, as in pushing or drawing a carriage, the impression produced upon our muscular sense does not correspond to the energy communicated per second, but to the energy communicated per inch of the motion. For experiment has proved that what appears to our muscular sense as a definite tension (in a cord, let us say) is associated with the communication of energy, to any mass of matter whatever, in direct proportion to the (linear) *space* through which it is exerted, altogether independently of the speed with which the mass may be already moving in the direction of the tension; so that in equal times energy is communicated in direct proportion to that speed. When there is no motion, no energy is communicated; and this would certainly not be the case if communication of energy corresponded to the *time* during which the tension was said to act.

12. The muscular sense is far more deceptive than any other, except, perhaps, that of touch. Conjurors, ventriloquists, perfumers, and cooks make their livelihood by practising on the imperfections of our senses of sight, hearing, smell, and taste respectively. But he who has tried the simple experiment of rolling a pea on the table between his first and second fingers, after crossing one over the other, will at once recognise the extreme deceitfulness of the sense of touch. And the muscular sense

well deserves a place beside it. So, as we know that there is but one pea, though the sense of touch vividly impresses us with the notion that there are two, we must be very wary when the muscular sense plainly suggests to us the notion of force as an objective reality.

13. Many of the terms which are now used in a strictly scientific sense had a humbler origin, having been devised entirely for the popular expression of common ideas. The term *Work* is a specially illustrative one. Thus, in a draw-well, the work done in bringing water to the surface would be reckoned at first in terms of the quantity of water raised :—two raisings of a full bucket lifting twice as much water as one. But then it was found that, for the same quantity of water raised, the work depended on the depth of the well :—doubled depth corresponding to doubled work. Again, if the bucket were filled with sand instead of water, more work was required, in proportion as sand is heavier than water. All these statements were soon found to be comprehended in the simple form :—the work done is directly proportional to the weight raised and also to the height through which it is raised. Here the indications of the muscular sense stepped in, and work came to have a general meaning, viz. the product of the so-called force exerted, into the distance through which it is exerted.

Had they not possessed the muscular sense, men might perhaps have been longer than they have been in recognising the important thing *potential energy* (§ 4); but when they had come to recognise it, they would have stated that when water is raised it gains potential energy in proportion as it is raised, and perhaps they might have found it convenient to use a single term for the rate at which such energy is gained per foot of ascent. This would probably not have been the word "Force," but it would have expressed precisely what the word force now expresses.

Then they would have recognised that when energy is transmitted by a driving-belt, the amount transmitted is (*ceteris paribus*) directly proportional to the space through which the belt has run. They might have invented a name for the rate of transmission per foot-run of the belt; they might even have called it the *tension* of the belt; but, anyhow, it would be precisely what is now called force.

Let us look at the matter from another point of view.

14. A stone, if let fall, gradually gains *kinetic energy*, or energy of motion, and experiment shows that the energy gained is directly proportional to the vertical space fallen through. Hence we have come to say that the stone is acted upon by a force (its *weight*, as we call it) whose amount is practically the same at all moderate distances from the earth's surface.

But, so far as we know the question scientifically, we can say no more than that the stone has potential energy (just as water in a mill-pond has *head*) in proportion to its elevation above the earth's surface; and consequently, by the conservation of energy, it must acquire energy of motion in proportion to the space through which it descends. *Why* it has potential energy when it is raised, and why that potential energy takes the first opportunity of transforming itself into kinetic energy:—thus requiring that the stone shall fall unless it be supported:—are questions to be approached later. (Chap. VII.)

15. That the statement above is complete, without the introduction of the notion of force, is seen from the fact that a knowledge of the kinetic energy acquired, after a given amount of descent, enables us to determine fully

the nature of the resulting motion even when the stone is *projected*, obliquely or vertically, not merely allowed to fall. The question is easily reduced to one of mathematics, or rather of *Kinematics*, and as such the non-mathematical student must, for the present, simply accept the statement as true.

And thus we have another of the many distinct and independent proofs that Force is a mere phantom suggestion of our muscular sense; though there can be no doubt that, in the present stage of development of science, the use of the term enables us greatly to condense our descriptions.

But it is a matter for serious consideration whether we do not connive at a species of mystification by thus employing, in the treatment of objective phenomena, a term for a mere sensation, corresponding to nothing objective:—even although it be employed solely to shorten our statements or our demonstrations.

Every one knows that matter (*e.g.* corn, gold, diamonds) has its price; so (as we saw in § 7) has energy. We are not aware of any case in which force has been offered for sale. To "have its price" is not conclusive of objectivity, for we know that Titles, Family Secrets, and even Degrees, are occasionally sold; but "not to have its price" is at least all but conclusive against objectivity.

We are in the habit of speaking of fresh air, sunshine, &c., though obviously objective, as *priceless!*

16. These introductory remarks have been brought in with the view of warning the reader that we are dealing with a subject so imperfectly known that at almost any part of it we may pass, by a single step as it were, from what is acquired certainty to what is still subject for mere conjecture.

An exact or adequate conception of matter itself, could we obtain it, would almost certainly be something extremely unlike any conception of it which our senses and our reason will ever enable us to form. Our object, therefore, in what follows, is mainly to state experimental facts, and to draw from them such conclusions as seem to be least unwarrantable.

17. But, for the classification of the properties of matter, whether our classification be a good one or not, it is necessary that we should have a definition of matter.

From what was said in last section it is obvious that no definition we can give is likely to be adequate. All that we can attempt, then, is to select a definition which (while not obviously erroneous) shall serve as at least a temporary basis for the classification we adopt.

18. Numberless definitions of matter have been proposed.[1] Here are a few of the more important :—

(α) That which possesses *Inertia* (§ 9).

(β) The *Receptacle* or *Vehicle of Energy* (§ 8).

(γ) Whatever exerts or can be acted on by *Force*.

(δ) Whatever can be perceived by our senses, especially the sense of Touch. This is closely akin to the well-known definition of matter as a *Permanent Possibility of Sensation*.

(ε) Whatever can occupy space.

(ζ) Whatever, in virtue of its motion, possesses Energy.

(η) Whatever, to set it in motion, requires the expenditure of Work.

(θ) [Torricelli, *Lezioni Accademiche*, 1715, p. 25.] La materia altro non è, che un vaso di Circe incantato, il quale serve per ricettacolo della forza, e de' momenti dell' impeto. La forza poi, e gl' impeti, sono astratti tanto sottili, son quintessenze tanto spiritose, che in altre ampolle non si posson racchiudere, fuor che nell' intima corpulenza de' solidi naturali.

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[1] A remarkable collection of such (now historical) speculations, due to Professor Flint, is given in *Appendix I.*

(ι) [The Vortex Hypothesis of Lord Kelvin.] The rotating parts of an inert perfect fluid; whose motion is absolutely *continuous*, which fills all space, but which is, when not rotating, absolutely unperceived by our senses.

19. The mutual incompatibility of certain pairs of these definitions shows that some of them, at least, must be of the so-called metaphysical species (§ 3).

(α), (β), (ζ), (η), above, have much in common, and, with further knowledge, may perhaps be found to differ in expression merely. At present, from want of information, we cannot be certain that any two of them are precisely equivalent.

Berkeley virtually asserted that all motion is produced by the direct action of spirits on matter. Even then, the statement (β) that matter is the receptacle or vehicle of energy holds good (but how then does energy exist in the spirit?).

But the statement that matter is whatever can exert force (γ) is to be rejected; though it was virtually introduced by Cotes in his Preface to the second edition of the *Principia*.

(δ) must be rejected, if only because there is *another thing* besides matter (in the physical universe) which we know of, and of course only through our senses (§ 1). But this is not all the error; for we get the notion of force through our muscular sense (§ 11), and force is not matter, not even a *thing*.

Torricelli's language is poetical, and therefore his statement (θ) must not be taken too literally. In his time, as in all subsequent time till well within the last half century, energy and force were very rarely distinguished from one another. Even now they are too often confounded.

(ι), the most recent of these speculations, has the curious peculiarity of making matter, as we can perceive it, depend upon the existence of a particular kind of motion of a medium which, under many of the definitions above, would be entitled to claim the name of matter, even when it is not set in rotation.

20. But as we do not know, and are probably incapable of discovering, what matter *is*, we must content ourselves for the present with a definition which, while not at least *obviously* incorrect, shall for the time serve as a working hypothesis.

We therefore choose (ϵ) above, *i.e.* we define, for the moment, as follows:

Matter is whatever can occupy space.

Experience has proved that it is from this side that the average student can most easily approach the subject, *i.e.* here, as it were, the contour lines of the ascent (§ 80) are most widely separated.

21. But this definition involves three distinct properties:—(1) the Volume, (2) the Form or Figure, of the space occupied; and (3) the nature or quality of the Occupation.

Hence the older classical works on our subject almost invariably speak of matter as possessing—(1) *Extension*, (2) *Form*, and (3) *Impenetrability*. It is mainly for the sake of the first of these, and the preliminary discussions which it necessarily introduces, that we have chosen the above definition as our starting-point.

22. Before we take these up in detail, however, it may be useful to devote a short chapter to a digression on some of the more notable of the hypotheses which have been propounded as to the ultimate structure of matter. We advisedly use the word *structure* instead of *nature*, for it must be repeated, till it is fully accepted, that the discovery of the ultimate nature of matter is probably beyond the range of human intelligence.

Another chapter, of a very miscellaneous character, will follow, devoted to the examination of some of the terms popularly applied to pieces of matter, and a rapid glance at the physical truths which underlie them. This is introduced to give the reader, at the very outset of his work, a general idea of its nature and extent.

CHAPTER II.

SOME HYPOTHESES AS TO THE ULTIMATE STRUCTURE OF MATTER.

23. The hard *Atom*, glorified in the grand poem of Lucretius, but originally conceived of, some 2400 years ago, by the Greek philosophers Demokritus and Leukippus, survives (as at least an unrefuted, though a very improbable, hypothesis) to this day. Newton made use of the hypothesis of finite, hard, atoms to explain why the speed of sound in air was found to be considerably greater than that given by his calculations; which were accurate in themselves, but founded on erroneous or, rather, incomplete data. But in this problem Laplace found the *vera causa*, and in consequence Newton's apparent support of the hypothesis of hard atoms is no longer available.

Many of the postulates of this theory are with difficulty reconciled with our present knowledge; some have been contemptuously dismissed as "inconceivable." But any one who argues on these lines becomes, *ipso facto*, one of the so-called metaphysicians.

Let us briefly consider the main statements of this theory, but without regard to the order in which Lucretius gives them.

24. Nature works by invisible things; thus paving-

stones and ploughshares are gradually worn down without the loss of any visible particles.

Reproduction [*i.e.* agglomeration of scattered particles so as to produce visible bodies] is slower than decay [*i.e.* the breaking up of bodies into invisible particles], and *therefore* there must be a limit to breakage, else the breaking of infinite past ages would have prevented any reproduction within finite time. Hence there exists a least in things [*i.e.* unbreakable parts or Atoms, "strong in solid singleness"].

But there is also void in things, else they would be jammed together, and unable to move. Here Lucretius takes the case of a fish moving in water, showing that void is necessary in order that it may be able to move. [Our modern knowledge of *circulation*, *i.e.* the motion of fluids in re-entrant paths, shows that this reasoning is baseless.]

There can be no third thing besides body and void. For nothing but body can touch and be touched; and what cannot be touched is void. [Here we have the germ of the erroneous definition of matter (δ) in § 18 above.]

The atoms are infinite in number, and the void in which they move [space] is unlimited.

They have different shapes; but the number of shapes is finite, and there is an infinite number of atoms of each shape.

Nothing whose nature is apparent to sense consists of one kind of atoms only.

The atoms move through void at a greater speed than does sunlight.

Besides this, there is a great deal of curious speculation as to how a vertical downpour of atoms [supposed to be a result of their weight] is, in some arbitrary way, made consistent with their meeting one another and agglomerating into visible masses of matter.

The basis of the whole of Lucretius' reasoning in favour of the existence of atoms lies in the gratuitous assumption that reproduction is slower than decay. This is by no means consistent with our modern knowledge, for potential energy of different masses [whether gravitational or chemical] is constantly tending to the agglomeration of parts, and on a far grander scale than that in which any known cause tends to decay or breaking up.

But if there be *hard* atoms, they must (in all known bodies) have intervals between them; for compressibility :—*i.e.* capability of having the component atoms brought more closely together :—is a characteristic of all known bodies. [Contrast this mode of arriving at the conclusion that "there must be void in things," with the erroneous mode employed by Lucretius.]

25. A refinement of this theory, mainly due to Boscovich, gets rid of the material atom altogether, substituting for it a mere mathematical point, towards or from which certain forces tend. It is supported by the assertion that we know matter only by the effects which it produces (or seems to produce), and therefore that, if these effects can be otherwise explained, we need not assume the existence of substance or body at all. This theory was, at least in part, accepted by so great an experimenter and reasoner as Faraday. It virtually substitutes force for matter as an objective *thing* (§ 2), and it essentially involves the heresy of distance-action (§ 10). But the fatal objection to which it is exposed is that it does not seem capable of explaining inertia, which is certainly a distinctive (perhaps the most distinctive) property of matter.

This theory must be regarded as a mere mathematical fiction, very similar to that which (in the hands of Poisson and Gauss) contributed so much to the theory of statical Electricity; though, of course, it could in no way aid inquiry as to *what* electricity is.

26. A much more plausible theory is that matter is continuous (*i.e.* not made up of particles situated at a distance from one another) and compressible, but intensely heterogeneous; like a plum-pudding, for instance, or a mass of brick-work. The finite heterogeneousness of the most homogeneous bodies, such as water, mercury, or lead, is *proved* by many quite independent trains of argument based on experimental facts. If such a constitution of matter be assumed, it has been shown[1] that gravitation alone would suffice to explain at least the greater part of the phenomena which (for want of knowledge) we at present ascribe to the so-called *Molecular Forces.* But it does not seem to be compatible with experimental facts; especially some of the simpler phenomena presented by gases. (§§ 55, 322.)

27. The most recent attempt at a theory of the structure of matter, the hypothesis of *Vortex Atoms*, is of a perfectly unique, self-contained character. Its postulates are few and simple, but the working out of anything beyond their immediate consequences is a task to tax to the utmost the powers of the greatest mathematicians for generations to come. A vortex filament, in a pefect fluid, is a true "atom;" but it is not hard like those of Lucretius; it cannot be cut, but that is because it *necessarily* wriggles away from the knife.

The idea that motion is, in some sort, the basis of what we call matter is an old one; but no distinct conceptions on the subject were possible until v. Helmholtz, in 1858, made a grand contribution to hydrokinetics in the shape of his theory of vortex motion.[1] He proved, among other entirely novel propositions, that the rotating portions of a continuous incompressible fluid, in which there is neither viscosity nor finite slipping, *maintain their identity*:—being thus for ever definitely differentiated from the non-rotating parts. He also showed that these rotating portions are necessarily arranged in continuous, endless filaments:—forming closed curves, which may be knotted or linked in any way:—unless they extend to the bounding surface of the fluid, in which alone they can have ends. Thus, to give ends to a closed vortex filament (*i.e.* to cut it) we must separate the fluid mass itself, of which it is a portion:—so that on Lord Kelvin's theory we must (virtually) sever space itself.

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[1] W. Thomson, *Proc. R.S.E.*, 1862.

Such vortex filaments (though necessarily of an imperfect character) are produced when air is forced to escape from a box, through a circular hole in one side, by sharply pushing in the opposite side. If the air in the box be filled with smoke, or with sal-ammoniac crystals, the escaping vortex ring is visible to the eye; and the collisions of two vortex rings, which rebound from one another, and vibrate in consequence of the shock, as if they had been made of solid india-rubber, are easily exhibited. Experimental results of this kind led Lord Kelvin[2] to propound the theory that matter, such as we perceive it, is merely the rotating parts of a fluid which fills all space. This fluid, whatever it be, must have inertia:—that is one of the indispensable postulates of v. Helmholtz's investigation; and the great primary objection to Lord Kelvin's theory is, that it explains matter only by the help of something else which, though it is not what we call matter, must possess what we consider to be one of the most distinctive properties of matter.

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[1] *Crelle*, 1858. Translated (by Tait) in *Phil. Mag.*, 1867.

[2] *Proc. R.S.E.*, 1867.

28. This theory is still in its infancy, and we cannot as yet tell whether it will pass with credit the severe ordeal which lies before it, when the properties of vortices (which must be discovered by mathematical investigation) shall be compared, one by one, with the experimentally ascertained properties of matter. As we have already said, this theory is self-contained; no new hypotheses can be introduced into it; so that it possesses, as it were, no adaptability, or capability of being modified, but must fall before the very first demonstrated insufficiency, or contradiction, if such should ever be discovered.

29. But the really extraordinary fact, already known in this part of our subject, is the apparently *perfect* similarity and equality of any two particles of the same kind of gas, probably of each individual species of matter when it is reduced to the state of vapour. Of such parts, therefore, whether they be further divisible or not, each species of solid or liquid must be looked on as built up. This similarity of parts, very small indeed but still of essentially finite magnitude, has been so well treated by Clerk-Maxwell that, instead of insisting upon it here, we give a considerable extract from one of his remarkable articles in *Appendix II.* below.

30. The further treatment of the subject of structure, involving the question of *how* the component parts (be they atoms or not) of bodies are put together, must be deferred to the end of the work. What has been said

above must be looked on as a mere preliminary sketch, not intended even to be fully understood until the experimental data, on which all our reasoning *must* be based, are brought before the reader as completely as our limits permit.

CHAPTER III.

EXAMPLES OF TERMS IN COMMON USE AS APPLIED TO MATTER.

31. Before we proceed to a more rigorous treatment of our subject, it may be well to consider what physical truth underlies each of some of the many adjectives in common use as applied to portions of matter, such as *Massive, Heavy, Plastic, Ductile, Viscous, Elastic, Rigid, Opaque, Blue, Coherent*, etc.

This course secures a twofold gain, so far as the beginner is concerned; for, *first*, he is introduced by it, in a familiar way, to some of the more important terms which are indispensable in scientific description; and *second*, he obtains a glance here and there through the whole subject of Natural Philosophy, because the programme before us is so vague as to leave room for innumerable digressions, each introducing some novel but important fact or property. But we must endeavour to be brief, for whole volumes would have to be written before this subject could be nearly exhausted.

32. Every one who has used his senses to any purpose knows, before he comes to the study of our science, a great many of its phenomena, among them some of the yet unexplained. But he knows, as it were, each by

itself, and only in its more prominent features; the analysis of the appearances or impressions which he has seen and experienced, and the explanation of the physical fact or process which underlies each of them, are absolutely necessary before he can understand the mode in which they must be grouped, and the reasons for such grouping.

33. Thus he knows that the moon keeps company with the earth, never receding nor approaching by more than a small fraction of the average distance. He also knows that the earth keeps, within narrow limits, at a definite distance from the sun. He has a general notion, at least, that the state of matters on the earth would become serious, as regards both animal and vegetable life, if we were to approach to even half our present distance from the sun, or recede to double that distance. But he would require to be a Newton if, without instruction, he could divine that these results are due to the very cause which keeps the bob of a conical pendulum moving in a horizontal circle.

He sees ripples running along on the surface of a pool, but requires to be told that their motion depends upon the cause which rounds the drops of water on a cabbage-blade, or in a shower, and which renders it almost impossible to keep a water-surface clean.

He sees what he calls a flash of lightning, but he requires to be told that what he sees is mainly particles of air heated so as to be self-luminous.

He looks at the stars and thinks he sees them as they are, but he requires to be informed that he sees even the nearest of them only as it was *three years ago* and that it may have changed entirely in the interval.

And he will certainly require to be informed, even with patient iteration, that air is made up of separate and independent particles:—the number of which in a single cubic inch is expressed by twenty-one places of figures, a multitude altogether beyond human conception:—a busy jostling crowd, each member of which darts about in all directions, impinging on its neighbours some *eight thousand million times per second.*

But when he has got so far, and has been told that this astounding information is as nothing to what we feel convinced that science can yet reveal, he cannot help marvelling alike at the arcana of physics, and at the patient efforts of genius which have already penetrated so far into the darkness shrouding its mysteries.

34. Take the terms *Massive* and *Heavy* as applied to a piece of matter, or the corresponding substantives, the *Mass* and the *Weight* of a body.

The terms are usually regarded as synonymous, but in their origin they are completely distinct. The one is a property of the body *itself*, and is retained by it without increase or diminution wherever in the universe the body may be situated. The other depends for its very existence on the presence of a *second body*, the Earth:—and *varies* in inverse proportion to the square of the distance from the earth's centre.

The destructive effects of a cannon-ball are due entirely to its mass and to the relative speed with which it impinges on the target. They have nothing to do with its weight, for they would be exactly the same (for the same relative speed) in regions so far from the earth, or other attracting body, that the ball had practically *no weight at all.*

When an engine starts a train on a level railway, or when a man projects a curling-stone along smooth ice, the resistance which either prime mover has to overcome is due to the mass of the body to be moved. Its weight, except indirectly through friction, has nothing to do with it. So when we *open* a large iron gate properly supported on hinges, it is the mass with which we have to deal; if it were lying on the ground and we tried to *lift* it, we should have to deal simultaneously with its weight and with its mass.

The exact proportionality of the weights of bodies to their masses, at any one place on the earth's surface, was proved *experimentally* by Newton, and is thus no mere truism, but an essential part of the great law of gravitation.

Thus a pound of matter is a definite amount, or mass, of matter, unchangeable whithersoever that matter may be carried. But the weight of a pound of matter, or a "pound-weight," as it is commonly called, is a variable quantity, depending upon the position of the body with respect to the earth; and changes (to an easily measurable amount) as we carry the body to different latitudes, even without leaving the sea-level.

35. The common use of the balance as a means of measuring out equal quantities of matter is justified by Newton's result; but the process is essentially an indirect one, for the balance tells only of equality of weight. If the earth were hollow at the core, the balance would cease to act in the cavity (§ 140). Bodies would preserve their masses there, but would be deprived of weight, at least so far as the earth is concerned.

To sum up for the present, the mass of a body is estimated by its inertia, and is taken as the measure of the amount of matter in the body; while the weight is an accidental property, connected with the presence of another mass of matter. But it is a most remarkable fact that under the same given external conditions the weight depends upon the quantity only, and not on the *quality*, of the matter in a body.

If a body, A, becomes heavy in consequence of the presence of another body, B, so in like wise does B become heavy in consequence of A's presence. And the weights of the two, each as produced by the attraction of the other, are exactly equal. Hence, if they be free to move, the *quantities of motion* (*i.e.* the momenta) produced in a given time are equal and opposite. [Newton's *Lex* iii. § 128.] But as the momentum is the product of the mass and the velocity, the parts of the velocities of the two bodies, due to their mutual gravitation alone, will be in amount *inversely as their masses*. Thus, though the weight of the whole earth, produced by the attraction of a stone, is exactly equal to that of the stone produced by the attraction of the earth, the consequent rate of fall of the earth towards the stone is less than that of the stone towards the earth in the same ratio that the mass of the stone is less than that of the earth, and is therefore usually so small as to escape observation. The moon, however, is a stone whose mass is not excessively smaller than that of the earth, and the consequences of the earth's fall towards the moon have to be taken account of in astronomy.

36. To properties such as mass, which depends on the size as well as on the material of a body, and weight which, in addition, depends on a second body, there correspond what are called *specific* properties, characteristic of the substance and independent of the dimensions of the particular specimen examined.

Thus the mass of a cubic foot of any kind of matter may be called its *specific mass*. But this quantity, *i.e.* the amount of matter in unit bulk, is usually expressed by the term *Density*.

The weight of a cubic foot of each particular kind of matter in any locality may be called the *specific weight*. But as this varies, though in the same proportion for all bodies, from place to place, we use instead of it the *ratio* of the weight of a cubic foot of the substance to that of a cubic foot of some standard substance. This is called the *Specific Gravity*. Pure water, at the temperature called 4° C. (its maximum-density point), is usually taken as the standard substance.

Newton's experimental result shows that the density and the specific gravity of any substance are proportional to one another, so that if the density of water at 4° C. be taken as unit-density, a table of specific gravities is identical with a table of densities. (§ 166.) But we must repeat, the coincidence is an experimental fact, not (as yet at least) in any sense a *truism*.

Specific gravity is, in general, much more easily measured with accuracy than is density, so that it is usually the property to be directly determined, the other simply following from it in consequence of Newton's discovery.

37. To vary the subject widely, let us now consider the term *Viscous* as applied to fluids. The contrasted adjective is usually taken as *Mobile*.

When a liquid partially fills a vessel, and has come to rest, it assumes a horizontal upper surface. If the vessel be tilted, and held for a time in its new position, the liquid will again ultimately settle into a definite position, with its surface again horizontal. Practically it occupies the same *bulk* in each of these positions. Hence the only change it has suffered is a change of *form*.

But this change of form is much more rapidly attained in some liquids than in others, even when they are of nearly the same density. Some (such as sulphuric ether) attain their equilibrium position so quickly that they retain energy enough to oscillate about it for some time before coming to rest; others (such as treacle) attain it only after a long time and, unless in great masses and when violently disturbed, do not oscillate but gradually creep to their final shape. Hence we call treacle viscous. To analyse this result let us consider (in a very elementary case, for the general analysis of the process requires higher mathematical methods than we can employ in a work like this) what is involved in *Shear*:—*i.e.* change of *form* of a body without change of *bulk*.

38. When water flows, without eddies, slowly in a rectangular channel of uniform width and depth, we know, by observation of particles suspended in it, that the upper parts flow faster than the lower, and (practically) in such a way that a column of the water, originally straight and vertical, inclines, as a whole, forwards more and more in the direction of its motion. Hence in a vertical section, along the middle of the channel, the particles originally forming the line ab in the figure will,

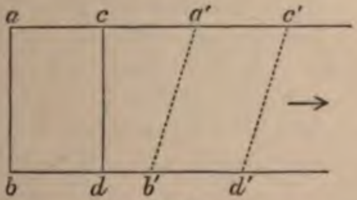

FIG. 1.

after the lapse of a certain time, be found approximately in the line $a'b'$. Similarly those which were originally in

cd parallel to ab, will be found in $c'd'$, parallel to $a'b'$, and so situated that $a'c' = ac$, and of course also $b'd' = bd$. The figures ad, $a'd'$, are thus parallelograms on equal bases and between the same parallels, and therefore equal in area. This shows that the water enclosed between vertical cross sections through ab and cd has the same volume as that between inclined sections (perpendicular to the sides) passing through $a'b'$ and $c'd'$. There has thus been change of form only in this mass of water, and we see that it has been produced by the *sliding* of every horizontal layer of the water over that immediately beneath it. [The same result follows even if $a'b'$ be not straight, for $c'd'$ will necessarily be equal and similar to it.] A good illustration of the nature of this kind of distortion will be seen in the leaves of an opened book, especially a thick one, such as the *London Directory*. It is often well exhibited by piles of copies of a pamphlet, or of quires of note-paper curiously arranged in a shop-window. Now when there is resistance to sliding of one solid on another we call it *Friction*. Thus the viscosity of a fluid is due to its internal friction, just as the slower motion at the bottom than at the top of the channel is to be ascribed to the friction of the liquid against the solid. [Another illustration of the subject is frequently furnished by the way in which we can judge of the direction of the wind from the mere *form* of detached clouds, as seen at a single glance. For such clouds, if originally nearly spherical, are distorted into ellipsoids whose longer axes overhang, as it were, the direction of motion.]

39. We now see *why* it is that disturbances of liquids gradually die away:—why the waves on a lake, or even on an ocean, last so short a time after the storm which

produced them has ceased. Also why winds (for there is friction in gaseous fluids as well as in liquids, though the mechanical explanation of its *origin* may not be quite the same) gradually die out. In either case the energy apparently lost is, as in the case of friction of solids, merely transformed into heat. We also see why it is that winds have the power of raising water-waves.

The stirring of water, or oil, and the measurement of the consequent rise of temperature when the whole had come to rest, the work done in stirring being also determined, was one of the processes by which Joule found, with great accuracy, the dynamical equivalent of heat.

40. It is very instructive to watch the ascent of an air-bubble in glycerine, and to compare it with that of an equal bubble in water. The experiment is easily tried with long cylindrical bottles, *nearly* full of different liquids, but having a small quantity of air under the stopper. When the bottle is inverted the bubble has to traverse the whole column.

The (apparent) suspension in water of mud, and exceedingly fine sand (to whose presence the exquisite colours of the sea and of Alpine lakes are mainly due), is merely another example of viscosity. So is the suspension of fine dust, and of cloud particles, in the air. Stokes[1] calculates that a droplet of water, a thousandth of an inch in diameter, cannot fall in still air at a much greater rate than an inch and a half per second. If it be of one-tenth of that size it will fall a hundred times slower, *i.e.* not more than one inch per minute! It is very remarkable that the resistance in such cases varies as the diameter of the drop. (See § 316.) With larger bodies, moving faster, the resistance is proportional to the sectional area.

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[1] On the Effect of the internal Friction of Fluids on the Motion of Pendulums. *Camb. Phil. Trans.* ix. (1851), eq$^{\text{n.}}$ (127).

41. Bodies are called *Elastic* or *Non-elastic*. Compare, for instance, the properties of a wire of steel with those of a lead wire; or of a piece of india-rubber and a piece of clay or putty. But the popular use of these terms is generally very inaccurate. The blame rests mainly with the ordinary text-books of science, which are (as a rule) singularly at fault with regard to the whole of this special subject, including even its most elementary parts.

Elasticity, in the correct use of the term, implies that property of a body in virtue of which it recovers, or tends to recover, from a deformation.

The phrase "tends to recover" is scarcely scientific; we should preferably say "requires the continued application of deforming stress (§ 128) to prevent recovery, entire or partial, from deformation."

Kinematics shows us that any deformation, however complex, is made up of mere changes of *bulk* and of *form*.

A distortion may therefore be wholly Compression, or wholly Shear (§ 37), or made up of these in any way. Hence there are two distinct kinds of Elasticity, viz. *Elasticity of Bulk* and *Elasticity of Form*. The former is possessed in perfection by all fluids, while the second is wholly absent. In solids both are present, but neither in perfection (except perhaps in very special cases, and then within very narrow limits).

Thus we see that, as a necessary preliminary to investigations on elasticity of bodies, we must study their capabilities of being distorted:—a whole series of properties, such as compressibility, extensibility, rigidity, etc.

This investigation is given in Chap. VIII., and its applications in Chaps. IX., X., XI. below.

42. In popular language, bodies are said to be *White*, *Black*, *Blue*, *Red*, etc. The investigation of the underlying scientific facts, on which these depend, is partly physical (and therefore within our scope), but also partly physiological. The subject is thus a somewhat complex one.

What do we mean by *White Light?* This is a question much more physiological than physical; dealing, as it does, with phenomena which are subjective rather than objective. Probably the true answer to it depends upon circumstances, or conditions, which may be varied indefinitely, and with them will necessarily vary what is described in terms of them.

Thus, in a room lit by gas, a piece of ordinary writing-paper, or of chalk, appears white :—at least if we have been in the room for some little time. But if, beside it, there be another piece of the same paper or chalk on which, through a chink, a ray of sunlight is allowed to fall (weakened, if necessary, so as to make the two appear of nearly the same *brightness*), we at once call the first piece of paper or chalk yellow, allowing the second to be *white*. Here we enter on a purely physiological question. In fact, if we accustom ourselves, for a sufficiently long time, to the observation of bodies in a room lit up only by burning sodium (which gives almost homogeneous orange light), we may ultimately come to regard bright bodies such as chalk, etc., as being white :—others, of course, being merely of different shades, or degrees of blackness. This, therefore, is foreign to our present subject. For all that, it furnishes us with the means of answering an important question somewhat different from that proposed above, but now a physical question :—viz. What do we mean by a *white body?*

43. Suppose *two* sources to exist in the room, giving different kinds of homogeneous light; one being incandescent sodium as before, the other incandescent lithium, which (at moderate temperatures) gives a homogeneous red light. Chalk and ordinary writing-paper will still appear as white bodies to an eye which has become accustomed to the light in the room; other bodies appear darker, but some are reddish, some of an orange tint.

And thus we obtain the idea that what we call a *white body* is one which sends to the eye, in nearly the same proportion as it receives them, the various constituents of the light which falls upon it; while a black body sends none; and coloured bodies send back light which, while (in general) necessarily made up of the same constituent as the incident light, contains them in *different proportions* to those in which they fell upon it. [It would only confuse the student were we here to refer to *Fluorescence.*]

44. Thus *white light* would seem to be a mere relative term. It is conceivable that the inhabitants of worlds whose sun is a blue star, or a red star (and there are many notable examples of such stars), may have their peculiar ideas of white light, formed from their own circumstances; as ours is formed from the light of our own sun, which is what, in contrast with these, we must call a yellow star.

However this may be, the discussion above has shown what is meant by a white body. A blue body is, by similar reasoning, one which returns blue rays in greater proportion than it does those of other visible light. It is therefore said to *absorb* the other rays in greater proportion than it absorbs the blue rays.

Now we are in a position to understand why blue and yellow pigments, mixed together, give green:—while a disc, painted with alternate sectors of the same blue and yellow, appears of a purplish colour when made to rotate rapidly. For the light given out by the rotating disc is a *mixture*, in the proportion of the angles of the sectors, of the kinds of light returned by the blue and yellow separately. But that which the mixed pigments send back has in great part penetrated far enough into the mass to run, as it were, the gauntlet of absorption by *each* of the separate components in turn, and therefore is finally made up of those rays alone which are not freely absorbed by either.

To this discussion we need only add, in illustration of the conservation of energy, that a body is always found to be *heated* in proportion to the amount of light-energy which it absorbs.

45. Shifting our ground again, we next take the words *Malleable*, *Ductile*, *Plastic*, and *Friable*, as applied to solid bodies.

All of these refer specially to the behaviour of solids under the action of forces which tend to change their *form*; for the change of *volume* of solids, even under very great pressures, is usually very small. The first three indicate that the body preserves its continuity while yielding to such forces, the fourth that it breaks into smaller parts rather than change its form. And, in popular use at least, the terms imply in addition that the body is not sensibly elastic.

46. The most perfect example of a malleable body is metallic gold. The gold leaf employed for "gilding," as it is called, is prepared by a somewhat tedious process, which requires a high degree of skill in the workman. The gold is first rolled into sheets thinner than the thin-

nest writing-paper (thus already showing a high amount of plasticity); next it is beaten out between leaves of vellum, till its surface is increased, and therefore its thickness diminished, some twenty-fold. A small portion of this fine leaf is then placed between two pieces of gold-beater's skin; and a more skilful workman, with a lighter hammer, again extends its surface twenty-fold. This operation can be *repeated* without tearing the thin film of metal, so great is its *tenacity*. (§ 226.)

Here we have one dimension (thickness) diminished in a marked manner, but the product of the other two dimensions (the surface of the leaf) is of course proportionally increased.

47. The action of the hammer may be practically viewed as equivalent to that of an intense pressure exerted through a very small volume, thus at every stroke applying a finite amount of energy. One portion of this is changed into heat in the hammer, the anvil, and the gold leaf; the rest is employed in doing work against the molecular forces of the gold, and thus altering its form.

To show that this is the true explanation of the observed effect, we may vary the experiment by subjecting a leaden bullet to the action of a hydrostatic press. A few strokes of the pump suffice to bruise the bullet into a mere cake. The process is essentially the same as that of gold-beating, but lead is by no means so malleable as gold.

48. This leads us, in our present discursive treatment of parts of our subject, to inquire how it is, that by means of such a machine as the Bramah press, a man can apply pressure sufficient to mould a piece of lead, whose shape he could scarcely alter to a perceptible amount by the direct pressure of the hand.

Here we have a first inkling of the *Function of a Machine.* A machine is merely a contrivance by which we can apply work in the way most suitable for the purpose we have in hand. Work (as a form of energy) is a real thing, whose amount is conserved. But we have seen that it can be measured as the product of two factors—the (so-called) force exerted, and the space through which it is exerted. Hence, because even when a machine is *perfect* it can give out only the energy communicated to it, if there be but one movable part to which energy is supplied and another by which it is given off, the simultaneous linear motions of these two parts must be in the inverse ratio of the forces applied to them, or exerted by them, in the direction of these motions respectively. Thus we are not concerned with the interior structure, or mode of action, of a perfect machine: all we need to know is the necessary relation of the *speeds* of the two parts or places at which energy is taken in and given out. This is a matter of kinematics, and can be made the subject of direct measurement when the machine is caused to *move*, whether it be transmitting work or not.

The statement just made is embodied in the vernacular phrase—

What is gained in power is lost in speed.

Objections may freely be taken to this form of words, but it is meant to imply precisely what was said above as to the action of a *perfect* machine.

If the machine be imperfect, as, for instance, if there be frictional heating during its working, the heat so produced represents some of the energy given to the machine, and the remainder of it is alone efficient.

49. A substance is said to be ductile when it can be drawn into very fine wires—*i.e.* when it admits of great exaggeration of one of its three dimensions (length) at the expense of the product of the other two (cross section). Wire-drawing is, essentially, a very coarse operation, for it has to be effected by *finite stages*, the wire being drawn in succession through a number of holes in a hard steel plate, in which each hole is a little smaller in diameter than the preceding one. The more nearly continuous the operation is made, the more tedious and therefore the more costly it becomes.

The associated tenacity and plasticity of silver render it one of the most ductile of metals. And an ingenious idea of Wollaston's enables us, as it were, to impart to other metals much of the ductility of silver. His idea may be briefly explained by analogy as follows. Suppose a glass rod, whose core is coloured, be drawn out while softened by heating, the diameter of the core is found to be reduced in the same proportion as is that of the rod. Thus, to obtain platinum wires much finer than could be procured by direct drawing, Wollaston suggested the boring of a hole in the axis of a cylindrical rod of silver, plugging the hole with a platinum wire which just fitted it, and then drawing into fine wire the compound cylinder. When this operation has been carried to its limit, practically determined by the ductility of the silver, the diameter of the platinum has been reduced nearly in the same proportion as that of the silver; and the silver may be at once removed from the fine platinum core by plunging the whole in an acid which freely attacks silver but has no effect on platinum.

50. Plasticity is shown, on the large scale, by many substances which, in hand specimens, appear fragile in

the extreme. Glacier-ice is one of these, but its behaviour is so closely connected with its thermal properties that we can only mention it here.

The whole earth, though its rock-structure appears so rigid, has been found to be more plastic (under the *tidal* attraction of the moon) than a globe of glass of the same size would have been.

But it is specially under the action of small but *persistent* forces that bodies, which are usually regarded as brittle or friable, show themselves to be really plastic. A good example of this is given by an experiment due to Lord Kelvin. Cobbler's wax is usually regarded as a very brittle body; yet if a thick cake of it be laid upon a few corks, and have a few bullets placed on its upper surface (the whole being kept in a great mass of water to prevent any but small changes of temperature), after a few months the corks will be found to have forced their way upwards to the top of the cake, while the bullets will have penetrated to the bottom.

51. For variety, let us next take the terms *Transparent*, *Translucent*, and *Opaque*.

These refer, of course, to the behaviour of a substance with regard to the passage of light through it. In common speech, a pane of ordinary window-glass is called transparent, while a piece of corrugated or of ground glass is translucent:—the latter transmits rays, no doubt, but with their courses so altered that they are no longer capable of producing distinct vision of the source from which they come. Consistency would require that the term translucent should also be applied to irregularly-heated air, or to a mixture of water and strong brine before diffusion has rendered it uniform throughout.

Translucent is hardly a scientific word, unless we choose to limit its application to heterogeneous bodies. In science we speak of the degree of transparency of a homogeneous substance; as, for instance, water more or less coloured, and employed in greater or less thickness. In such cases, besides the inevitable surface-reflection, there is more or less absorption; and the percentage of any definite kind of incident light which unit thickness of the substance transmits is called its transparency for that kind of light.

Opacity may arise from either of the two causes just mentioned. Light may *enter* a body, and be unable to proceed farther, as is the case with lamp black. Or it may fall on a highly polished surface, such as thinly silvered glass, and be in great part reflected without entering.

In the former case it is said to be absorbed; and, when this happens, the absorbing body is raised in temperature. The incident energy is converted from the radiant form into that of heat.

In the latter case part only can enter the body; and, if it meet in succession other reflecting surfaces in sufficient number, practically the whole of it may be reflected. This is the case with a heap of pounded glass, a cloud, a mass of snow, or of froth or foam. All of these materials are transparent, but they reflect *some* of the incident light; and, in consequence of the multiplicity of surfaces which the light has to encounter, the greater part of it is reflected before it has penetrated deeply into the mass. Hence the whiteness and brightness of snow and clouds in full sunshine.

52. We have here an excellent opportunity of calling the student's attention to the distinction:—a very profound one:—between *Heat* and *Temperature*.

For we have seen that energy, in the form of light, when absorbed, becomes heat in the absorbing body, and thus raises its temperature. But if the *same* quantity of heat had been given to a body, of the same nature but of twice the mass, the rise of temperature would have been only *half* as great. The very form of words here used shows at once how different are the meanings of the words temperature and heat. For the quantity of heat (so much energy, a real thing) is perfectly definite, but the effect it produces on the temperature (a mere *state*) depends on the quantity and quality of the mass to which it is communicated.

Heat is therefore a thing, something objective; temperature is a mere CONDITION of the body, with which the heat is temporarily associated; a condition which in certain cases determines the physical state of the body itself, and in all cases determines its readiness to part with heat to surrounding bodies or to receive it from them.

Heat may, in this connection, but only for illustration, be compared with the air compressed into the receiver of an air-gun; temperature would then be analogous to the pressure of that air. Neither of two receivers would (except by diffusion, with which we are not at present concerned) give air to the other, when a pipe is opened between them, if the pressure were the same in both; but air would certainly flow from the receiver in which the pressure is greater to the other; and this, altogether independently of the relative capacities of the two receivers, or the consequent amounts of their contents.

53. As another example, take the terms *Cohesive*, *Incoherent*, *Repulsive*.

A lump of sandstone has considerable tenacity, which, of course, is to be ascribed to those molecular forces of which we spoke in § 26. But when, in virtue of its friability, it has been pounded down into sand, it becomes an incoherent powder. And we know that it must at some time previously have been in this form, for it often contains fossil plants or fish, and it may even have preserved (perhaps for a million or more of years) records of surface-disturbance in the form of dents made by rain or hail, or by the feet of birds or reptiles. When a sufficiently deep layer of sand is deposited, by drifting or otherwise, above this portion, its loose particles are brought by the consequent pressure so close together that their molecular forces once more come into play.

The graphite, or plumbago, which forms the material for the finest drawing-pencils, is a somewhat rare and valuable mineral. In cutting it up into "leads," however carefully, a considerable portion is reduced to powder—*i.e.* sawdust. But if this powder be exposed, in mass, to pressure sufficient to bring its particles once more within the extremely short mutual distance at which the molecular forces are sensible, these forces again come into play, and the powder becomes a solid mass, which can in turn be sawn into "leads" for a somewhat inferior class of pencils.

The whole of this part of the subject, especially as regards liquids, will be fully treated later, so that we need not further consider it here.

54. But let us contrast, with the behaviour of the particles of a solid or a liquid, that of the particles of a gas or vapour. Such substances require to be subjected to external pressure in order to prevent their particles from being widely scattered. When a small quantity of air is allowed to enter an exhausted receiver it dilates so

as to occupy with practical uniformity the whole interior of the receiver, however large that may be.

This result was, naturally enough, at first ascribed to a species of repulsion between the various particles; but the notion was found to be an erroneous one. For the effects of a true repulsion, capable of producing the practically infinite dilatation already spoken of, could not all be consistent with the corresponding observed results. The mode of departure from them depends upon the law according to which the repulsion may be supposed to vary with the distance between two particles. Some assumed laws would give as a consequence that the particles would all be driven to the *sides* of the vessel, leaving the interior void. Others would require that the pressure should change in value if we were to take half the gas and confine it in a vessel of half the content. Others would make it different at different parts of the surface unless the vessel were truly spherical, etc. etc.

The true explanation of the phenomenon becomes obvious to us when we apply heat to the gas. For it then appears that the pressure requisite to maintain the whole at a constant volume increases as the temperature is raised; and thus that heat is, in some way, the *cause* of the pressure.

55. Hence we are led to what is called the *Kinetic Theory* of gases, whose fundamental assumption (§ 33) is that the particles dart about in all directions (with an average speed which is greater the higher the temperature), impinging on one another, and also upon the sides of the containing vessel. This continued series of very small but very numerous impacts (each, by itself, absolutely escaping observation) is perceived by our senses as the so-called "pressure" exerted by the gas. Experiment

shows that, when a gas is confined in a vessel of definite size, the changes of its pressure are nearly proportioned to the changes of temperature, as measured by a mercury thermometer, whether these changes be in the direction of a rise or a fall. If we assume, for a moment, that this statement is true for all ranges of temperature, even beyond those attainable in experiment, it leads us to the very important question:—*At what temperature does the pressure of a gas vanish?*

Calculations carried out in the above way showed that, under the assumption just mentioned, all of the (so-called) permanent gases cease to exert pressure at one common temperature (about – 273° C.). Thermodynamical theory comes to our assistance and shows that the above guess is not far from the truth:—that a body, cooled to – 274° C., cannot be cooled any farther; that it then is, in fact, totally deprived of heat.

We might, therefore, fancy that a gas, if it could be brought to this temperature, would be reduced to a mere layer of incoherent dust or powder, deposited by gravity on the lower surface of the containing vessel. But experiment has shown that gaseous particles, even while in motion, if only close enough together, exert mutual molecular forces, so that the result (on any gas) of the conditions above specified would probably be its assuming a liquid or even a solid form. In fact Andrews has shown that, for each particular substance, there is a temperature (usually called the *Critical Point*) such that, while the substance is at any higher temperature it is necessarily a gas, in the sense that it cannot be liquefied by any pressure, however great. At *any* lower temperature it can be liquefied by the application of sufficient pressure, and is therefore to be regarded as a true vapour.

56. We speak of bodies as *Hard* and *Soft*. These are barely scientific terms; because, unless they are strictly defined, they may bear a great variety of meanings.

Thus, for instance, we have the mineralogist's *Scale of Hardness*, which is often of great practical value in field-work. For there are numerous instances in which two quite different minerals (sometimes a very valuable and a very common one) are almost undistinguishable from one another so far as colour, density, and crystalline form are concerned. Chemical tests (even the comparatively coarse blowpipe tests), though they would settle a question of this kind at once, are not readily applied in the field. Hence the use of the scale of hardness, in which minerals are so arranged that every one can scratch the surface of any other which is lower in the scale. By carrying a set of *twelve* small specimens only, of selected minerals, the finder of a doubtful crystal can readily determine its rank among them as regards scratching; and can thus often settle in a moment what would otherwise require some time, even with the facilities of a laboratory.

In such a scale diamond, of course, stands at the top, while native copper, one of the *toughest* of substances, is far below it.

But if we were to test relative hardness by some other method, say by blows of a hammer, we should be led to arrange our specimens in a very different order. The scale above spoken of is, therefore, by no means a scientific one; though, as we have seen, it may often give easily some useful information.

CHAPTER IV.

TIME AND SPACE.

57. We begin with an extract from Kant, who, as mathematician and physicist, has a claim on the attention of the physical student of a very different order from that possessed by the *mere* metaphysicians.

"Time and space are two sources of knowledge, from which various *à priori* synthetical cognitions can be derived. Of this pure mathematics give a splendid example in the case of our cognitions of space and its various relations. As they are both pure forms of sensuous intuition, they render synthetical propositions *à priori* possible. But these sources of knowledge *à priori* (being conditions of our sensibility only) fix their own limits, in that they can refer to objects only in so far as they are considered as phenomena, but cannot represent things as they are by themselves. This is the only field in which they are valid; beyond it they admit of no objective application. This peculiar reality of space and time, however, leaves the truthfulness of our experience quite untouched, because we are equally sure of it, whether these things are inherent in things by themselves, or by necessity in our intuition of them only. Those, on the contrary, who maintain the absolute reality of space and time, whether as subsisting or only as inherent, must come into conflict with the principles of experience itself. For if they admit space and time as subsisting (which is generally the view of mathematical students of nature), they have to admit two eternal infinite and self-subsisting nonentities (space and time), which exist without their being anything real only in order to comprehend all that is real. If they take the second view (held by some metaphysical students of nature), and look upon space and time as relations of phenomena, simultaneous or successive, abstracted from experience, though represented confusedly in their abstracted form, they are obliged to deny to mathematical propositions *à priori* their validity with regard to real things (for instance in space), or at all events their apodictic certainty, which cannot take place *à posteriori*, while the *à priori* conceptions of space and time are, according to their opinions, creatures of our imagination only."[1]

On matters like these it is vain to attempt to dogmatise. Every reader must endeavour to use his reason, as he best can, for the separation of the truth from the metaphysics in the above characteristic passage.

58. We must now take up, as indicated in § 21, the property *Extension*, which is one of those expressly included in our provisional definition of matter.

It implies that all matter has *volume*, or bulk. The thinnest gold leaf has finite thickness, the finest wire has a finite cross section.

In popular language this is recognised by the use of the associated terms *length*, *breadth*, and *thickness*.

In other words, the term extension recognises the essentially *Tridimensional* character of space.

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[1] *Critique of Pure Reason*. Max Müller's Translation.

Why space should have three dimensions, and not more nor less, is a question altogether beyond the range of human reason. Only those who fancy that they know what space is, would venture (at least after well considering the meaning of their words) to frame such a question.

59. The proof that our space has essentially three dimensions is given in its most conclusive form by the statement, *based entirely upon experience*, that

To assign the relative position of two points in space, three numbers (*of which one at least must be a multiple of the unit of length*) *are necessary, and are sufficient.*

It is an easy matter for us, accustomed to tridimensional space, to imagine one or more of its dimensions to be suppressed. In fact so-called *Plane Geometry* is the geometry of one particular kind of two-dimensional space; *Spherical Trigonometry* that of another. We cannot well speak of the *geometry* of space of one, or of no dimensions; but the idea we should thus attempt to express is a correct one, though the term is inappropriate.

When, however, we try to conceive space of four or more dimensions, we are attempting to deal with something of which we have not had experience; and thus, though we may by analogy extend our analytical and other processes to an imagined space, in which the relative position of two points depends on more than three numerical data, we can form no precise idea of how the additional dimensions would present themselves to our senses or to our reason.

A few remarks on this subject will be made at the end of the chapter.

60. Space of no dimensions is a geometrical point, of which nothing further can be said.

61. Space of one dimension:—let us call that dimension *Length*:—is a mere geometrical line which may be curved or straight. But to be sure of the existence of *this* characteristic, and to understand its true nature, we must have cognisance of space of *two* dimensions if it be a plane curve, of *three* if it be tortuous. The study of all the properties of space of one dimension, though an excessively simple affair, is of very great intrinsic importance, besides being a necessary step towards that of the higher orders. We will, therefore, treat it so fully that a far less amount of detail will be necessary when we come to two and to three dimensions.

62. Every one, whether he be aware of the fact or not, is acquainted by experience with at least the *elements* of this subject. Suppose, for instance, we take as our one-dimensioned space any one of the roads or railways leading from Edinburgh to London; which we will, for the moment, suppose to be straight, and to run due south. The mile-stones, set up at equal distances along the road, mark the positions of various points in terms of the one dimension, length, which is alone involved, or, rather, to which for the present we restrict our consideration. And a *Gazetteer* or a *Railway Guide* gives us the positions of the towns or stations along the road or line: the position of each being fully described by a *single number*, understood as a multiplier of a mile or of some other specified unit of length, and with a qualification which will presently be introduced.

But these numbers refer to the distance from some assumed starting-point, or *Origin* as it is technically called; say, in this case, London. Thus we find in an old Road Guide, for the particular one-dimensioned space called the *East Coast Route*, a column of data from which we extract the following:—

	Miles.
London	0
York	196
Berwick	337
Edinburgh	395

Fractions of miles are omitted, to avoid mere *arithmetical* complication.

From this table, by ordinary subtraction, we form a list, as below, of the lengths of what we may call the various *stages* of the route. Thus—

	Miles.
London to York	196
York to Berwick	141
Berwick to Edinburgh	58

It will be seen that, in this list, the origin from which each number is measured is *the first named* of the two corresponding places, and the number itself is found by subtracting, in the first list, the number corresponding to the first of the two places from that corresponding to the second.

63. Now let us at once take the *only* step which presents any difficulty. Choose York as our origin, and boldly apply the rule just given, no matter what the consequences may be. The result is—

	Miles.
London	-196
York	0
Berwick	141
Edinburgh	199

Here there is no difficulty whatever in understanding the numbers for Berwick and for Edinburgh. They are, as before, the numbers of miles by which Berwick and Edinburgh are separated from York. Also the number for London, when York is the origin, differs from that for York, when London is the origin, only *by change of sign.*

So that we at once recognise the meaning of the negative sign as applied to a length in our one-dimensional space:—it measures the length in the *opposite* direction to that in which a positive length is measured.

The necessity for this convention, and its extreme usefulness, were early recognised in Cartesian geometry, but they had long before been applied in common arithmetic as well as in algebra.

Perhaps the simplest view we can take of the subject is that afforded by a man's "balance" at the bank. So long as this is on the right side (*i.e. positive*) he can draw any less amount and still be on the credit side; if he overdraws (*i.e.* takes more out of the bank than his balance), the difference is *negative*, and he is to that amount indebted to the bank.

64. In the first of the three little tables above, all the places involved lay to the *north* of the origin (London), and were all *therefore* affected by the same sign (which we happened to take as +). When York was taken as origin, Berwick and Edinburgh were to the north, and their numerical quantities were still +. But London being to the south, had a − number.

It would be easy to give multiplied examples of this, but they are unnecessary. The only additional comments which we need make are these:—

(1.) When the northern direction along a line was called +, the southward necessarily became −. Similarly had we chosen southward as +, northward would have become −.

(2.) We chose for our special example a northward-running line, but we might equally well have chosen an eastward one, etc. Hence pairs such as N. and S., E. and W., up and down, etc., must be regarded as having their members contrasted exactly as are the + and − of Algebra or of Analytical Geometry.

And, just as a *displacement* in either direction along a line may be regarded as +, while a displacement in the opposite direction must then be regarded as −, so it is with rates of motion, *i.e.* *Speeds*, in space of one dimension.

Thus the relative speed of two trains running northward, A at 60 miles an hour, B at 40, is 20 miles an hour northward as regards A seen from B, and 20 southward as regards B seen from A; so if A be moving southward at 60 miles an hour, and B northward at 40, the speed of A with regard to B is 100 miles per hour southward, and of B with regard to A 100 miles per hour northward.

The idea of speed, as so many units of linear space described per unit of time, is a complex one:—involving both of the fundamental ideas. We express this by saying that its *Dimensions* are

$$\left[\frac{L}{T}\right].$$

This implies that, in whatever proportion we increase our unit of length, the measure of a speed is diminished in that proportion:—while it is increased in the same proportion as that in which the unit of time is increased.

Thus a speed of 5280 feet per second is but 1 mile per second; while a speed of 1 foot per second is 60 feet per minute.

65. A precisely similar distinction (as to + and −) is

observed when our one-dimensional space is a *curved* line;—take for example the orbit of a planet. To describe fully the position of the planet, when the orbit is given, *one* number alone (say the *angle-vector*, the angle which the *radius-vector*, or line joining the centres of planet and sun, makes with some fixed line in the plane of the orbit) is required. This, however, must again be qualified as + or −. (In the case of angles, we agree to call them + when they are measured in the *opposite* direction to that of the motion of the hands of a watch; that is, when they are described in the same sense as that in which the *northern* regions of the earth turn about the polar axis.) *Angular velocity* in one plane (*i.e.* rate at which the radius-vector turns) is similarly characterised.

In *all* cases where motion is restricted to one line the same thing holds. Thus the position of a pendulum is at every instant completely assigned by the angle the rod makes with the vertical, provided we are also told on which side the displacement is.

The record kept by a self-acting tide-gauge gives at any instant the elevation or depression (again + and −) of the water above or below the mean level. Similarly with registering barometers, thermometers, etc. But, for the full appreciation of the indications of these records, they are usually made in *two* dimensions by the use of an important principle which will presently be explained. (§ 68.)

66. In what precedes we have been dealing with a kind of space in which the only displacements are forward or backward; nothing is possible (nor even conceivable) sideways or upwards.

This characteristic applies to *Time*, as well as to space of one dimension, and therefore we should expect to find, as we do find, that (with the necessary change of a word or two) all that has just been said with reference to relative *position* is true of events in time, as well as of points in one-dimensional space. There is no such thing as motion or displacement in time, so that *this* part of the analogy is wanting. Every event has its definite epoch, for ever unalterable. And of course there is no going sideways or upwards, as it were, out of the one-dimensional course of time.

Thus we find that to assign definitely the position of an event in time, provided our origin is assigned, all we need know is a single number (a multiplier of the time-unit) with its *sign*, + or −, signifying time after or time before that origin.

Our usually adopted origin is the Christian era, and we speak of 1894 A.D. as the present year, while the date of the battle of Marathon is recorded as 490 B.C. The difference between the characteristics A.D. and B.C. is of precisely the same nature as that between north and south, or + and −.

Hence, if we wish to find the interval between the present time and the battle of Marathon, we have to *subtract* + 1894 (the position of the new origin) from − 490. The result is − 2384, *i.e.* Marathon was fought 2384 years *ago*. Thus to change the origin, or epoch, we must perform precisely the same operation as that which gave us the table in § 63, from the first table in § 62. Similarly, to change our system of chronology to the year of the world (designated by A.M.) or to the old Roman (marked A.U.C.), all we need do is to subtract from each date (A.D. or B.C., regarded as + and − respectively) the assumed date of the creation of the world

(4004 B.C.) or of the foundation of the city of Rome (753 B.C.).

We need say no more on such a matter. Every intelligent reader can make new and varied examples for himself.

67. Passing next to space of two dimensions, whether plane or spherical, we see at once from a map, or a globe, that the position of a place is given by two numbers, its *Latitude* and *Longitude.* But each of these has to be qualified for definiteness by the + or − sign, or something equivalent. Thus we have N. or S. latitude, and E. or W. longitude.

But there are two methods, specially applicable to the plane, which deserve closer attention in view not only of their intrinsic usefulness, but also of their bearing on the general question of tridimensional space. These are known in geometry as *Rectangular* and *Polar* coordinates.

68. In the first we assume two reference lines at right angles to one another, both passing through the origin, and assign the position of a point by giving its distances from these two lines. These distances are looked on as drawn *towards* the point from either line, and each therefore changes sign when the point is taken on the *other side* of the corresponding reference line. This is symbolised in the cut. Ox, Oy are the two reference lines, O the origin. The perpendiculars PM, PN, let fall from P on these lines, completely, and without ambiguity, define its position. For if we know OM or NP, the x of P, *i.e.* its distance from Oy, that condition alone limits our choice for P to points lying in PM, a line drawn parallel to Oy and everywhere at the assigned distance, x, from it. Similarly, y being given as ON or MP, the choice of points is

limited to those on the line NP, all of which have this property.

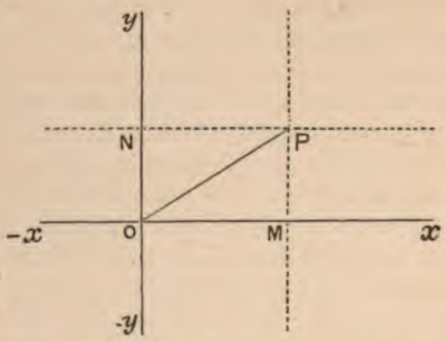

FIG. 2.

But two lines at right angles to each other must intersect, and in one point only. Thus the point P is determined by the conditions without ambiguity.

If P lie to the left of Oy, its x is negative; if below Ox, its y is negative. The lettering in the cut, at the ends of the lines bounding the four quadrants, shows at a glance the signs of x and y when P is situated in any one of them.

In general, any given relation, between the x and y of a point, limits its position to a definite *Curve* in the plane of the reference lines. It is often very convenient to represent such a relation by a curve; and, in fact, most self-registering instruments actually trace such a curve for us. Thus, if intervals of time (as OM) measured from a definite instant (represented by O) be laid off along Ox, with the corresponding heights of the thermometer, barometer, tide, etc., erected as perpendiculars (MP) at their extremities, we have (as the *Locus* of P) a curve showing the mode in which temperature, pressure of the atmosphere, etc., change as time goes on. But such curves can be traced by a pencil attached to the instrument, or by photographic processes, on a long band of paper which is drawn horizontally past it, at a uniform rate, by clockwork.

69. In the second method mentioned in § 67 the data are the length of OP (the radius-vector), and the magnitude of $<$MOP (the angle-vector), § 65. These are usually denoted by r and θ, respectively. Here r is always taken as a positive (or rather a *signless*) quantity, while θ is positive if it be measured round from Ox *counter-clockwise*.

This is the method adopted by a surveyor when, with a chain and a theodolite, he measures a field. His reference line, Ox, is usually given by a magnetic needle attached to the theodolite. He measures the angle xOP and the distance OP, P being a corner (let us say) of the field. These two data, determined for each prominent part of the boundary, enable him to *plot* the field; and therefore contain all the necessary numerical data for calculating its area, etc.

It is also the method usually employed in dealing with orbital motion of any kind in one plane.

Comparing the two methods, we see that the *directed line* OP may be resolved (as it is called) into OM and MP, lines in directions perpendicular to one another. Also that this resolution, in any direction, is effected by means of the cosine of the angle involved.

$$\text{For}\quad x=\mathrm{OM}=\mathrm{OP}\cos\overset{<}{x\mathrm{OP}}=r\cos\theta,$$

$$y=\mathrm{MP}=\mathrm{OP}\cos\overset{<}{y\mathrm{OP}}=r\sin\theta.$$

It is clear that, though we have hitherto spoken of O and P as the simultaneous positions of *two* points, we may look on them as successive positions of *one* (moving) point. If we look on the displacement as having been produced uniformly, and in one second, it represents in magnitude and direction the *Velocity* of the moving point; and OM, MP represent, on the same scale, its resolved parts or components, parallel to Ox and Oy.

70. As examples, we give one or two results which will be specially useful to us in later chapters.

If a point be moving, in any manner whatever, we may consider its velocity alone, independent altogether of the actual path pursued. Here we are introduced to a new idea, that of *Acceleration*. For, as velocity is rate of change of position, acceleration is rate of change of velocity.

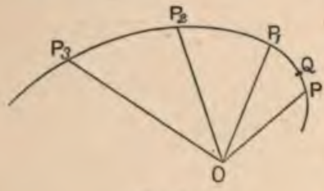

FIG. 3.

Take any fixed point, O, and let OP represent, in magnitude and direction, the velocity of the moving point. After one unit of time let the velocity be represented by OP$_1$; after two units, by OP$_2$; and so on. It is clear that all the points P, P$_1$, P$_2$, etc., lie on some definite curve, which will be the more accurately traced the greater the number of points we obtain in any assigned portion of it; *i.e.* the smaller we assume our unit of time. If the motion whose properties are thus studied be that of a particle of *matter*, this curve (which is called the *Hodograph*) is necessarily continuous, for the velocity cannot alter by starts (§ 120) either in magnitude or in direction. And, as OP passes to a proximate position, OQ, by having a velocity PQ compounded with it, the *Acceleration* of OP is the velocity with which P moves in its curve. If the path be a plane curve, the hodograph is also plane.

This construction enables us at once to solve a number of elementary problems in kinematics, which will be of great use to us in the sequel.

In § 64 above, we showed that the dimensions of speed (V) are

$$[V]=\left[\frac{L}{T}\right].$$

In precisely the same way we see that those of acceleration (A) are

$$[A]=\left[\frac{V}{T}\right]=\left[\frac{L}{T^2}\right].$$

Thus the numerical measure of acceleration is diminished in proportion as the unit of linear space is increased:—but is increased in the duplicate ratio of that of the time unit.

An acceleration of 1 foot per second, per second, is obviously the same as 3600 feet per minute, per minute.

71. Suppose a point to move uniformly, with speed V, in a circle of radius R. OP in the hodograph (Fig. 3) has constant length V, and its direction rotates uniformly. Hence the hodograph is another circle, also uniformly described in the same sense (*i.e.* clockwise or counter-clockwise), and in the same period of time. Hence the speed of P must be such that it describes a circle of radius V, in the time that a point whose speed is V takes to go round a circle of radius R. It must, therefore, be V^2/R. Also the direction of this speed is perpendicular to OP, and therefore *along* the radius of the first circle. And its direction is *towards* the centre of that circle,

because both circles are described clockwise, or counter-clockwise.

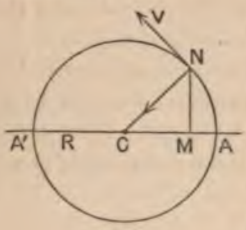

FIG. 4.

Let, now, the figure represent the circle of radius R, and draw any diameter, ACA'. Then N moves round with speed V, and the acceleration of its motion is V^2/R along NC. Remembering that accelerations and velocities are resolved like lines, we see that if NM be drawn perpendicular to AA', the speed of the point M along MC will be

$$V\frac{MN}{NC},$$

and its acceleration along MC, and *towards* C, will be

$$\frac{V^2}{R}\frac{CM}{CN}=\frac{V^2}{R^2}CM.$$

The motion of M, thus defined, is called *Simple Harmonic*. It obviously consists in a vibration back and forward along the line AA', the speed being greatest at C, and vanishing at A and A'. The special characteristic is that the acceleration is always directed towards C, and is *proportional* to the displacement of M from that point.

72. If we use Newton's *Fluxional Notation*, in which the rate at which a quantity increases per unit of time is expressed by putting a dot over the symbol for that quantity, a second dot placed over it will signify the rate at which that rate increases, and so on.

Thus, if CM above be denoted by x, the speed of M is $\dot{x}$, and its acceleration is $\ddot{x}$. And we see at once from the result of last section that

$$\ddot{x}=-\frac{V^2}{R^2}x,$$

the negative sign being prefixed because while x is directed to the right in the figure, the acceleration is directed to the left, and conversely. Whenever, in future chapters, we meet with a relation of this kind, we will, therefore, interpret it as expressing simple harmonic motion. The multiplier of the right-hand side depends only on the *ratio* of V to R:—what is called (§ 65) the angular velocity of the radius-vector CN. If we denote this by ω, the equation may be written

$$\ddot{x} = -\omega^2 x;$$

and this belongs to all simple harmonic motions, whatever be their range of vibration, provided the angular velocity in the corresponding unform circular motion be ω, or the period of a complete revolution $2\pi/\omega$. Any such motion is fully described by

$$x = a \cos.(\omega t + \alpha),$$

where a and α are absolutely arbitrary.

73. The result above was obtained by projecting uniform circular motion on a diameter of the circle, or, what comes to the same thing, on a plane *perpendicular* to the plane of the circle.

But an exceedingly interesting result is obtained by projecting the circular motion on any other plane. In orthogonal projection equal areas are projected into equal areas, and a circle is projected into an ellipse whose centre is the projection of the centre of the circle.

Hence the projection gives motion in an ellipse, the radius-vector drawn from the *centre* of the ellipse tracing out equal *areas* in equal times, and the acceleration being still directed inwards along the radius-vector, and still bearing the same proportion to it.

74. Another extremely useful result may be obtained

by supposing the unform angular velocity in the circle to be maintained, but with a continual *shrinking* of the radius at a rate measured (per second) by κ times its length at each instant.

The velocity of the moving point is thus made up of two components, one along the circle, the other along the radius, each proportional to the radius. Hence the path is a spiral which makes a constant angle with the radius, what is called the *Equiangular*, or *Logarithmic, Spiral.* The radius-vector still revolves uniformly.[1]

Let PR be the spiral, SP any radius. Then, if PT be

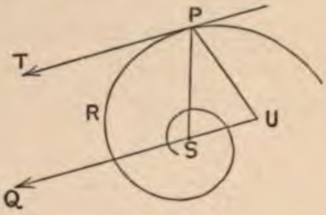

Fig. 5.

the velocity of P, and α the (constant) angle between its direction and that of PS, we see at once that

$$\text{PT} \sin \alpha = \omega \text{SP}, \quad \text{PT} \cos \alpha = \kappa \text{SP},$$

whence

$$\kappa = \omega \cot \alpha.$$

If SQ be equal and parallel to PT, Q is a point in the hodograph. But as PT, and therefore SQ, is *proportional* to SP, and the angle QSP is the supplement of α, the hodograph is the same spiral rotated through a given angle, and altered in its linear dimensions by the factor $\left(\frac{\omega}{\text{in}\ \alpha}\right)$. Thus the hodograph of the hodograph is another similar spiral, again turned through the same angle, and with its radii altered in the ratio $\left(\frac{\omega}{\sin \alpha}\right)^2$. If PU be drawn, making an angle α with TP produced backwards, and meeting QS in U, it will therefore be the *direction* of acceleration at P.

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[1] *Proc. R.S.E.*, December 19, 1867.

But PU may be resolved into PS, SU, the first of which is along the radius-vector, the second parallel to the tangent at P. The parts of the acceleration in these directions are, respectively,

$$\left(\frac{\omega}{\sin \alpha}\right)^2 \mathrm{PS}, \quad \text{and } 2\left(\frac{\omega}{\sin \alpha}\right)^2 \mathrm{SP} \cos \alpha.$$

The latter of these, by the first equation above, may be written as

$$2\left(\frac{\omega}{\sin \alpha}\right)^2 \mathrm{PT} \frac{\sin \alpha \cos \alpha}{\omega} = 2\omega \cot \alpha . \mathrm{PT} = 2\kappa \mathrm{PT}.$$

Hence the motion of P is due to an acceleration along, and proportional to the length of, PS, and another along, and proportional to the length of, TP.

And of course the resolved part of the motion along any line in the plane possesses the same characteristics. If x represent the distance between the projections of S and of P, on such a line, we see at once that we have

$$\ddot{x} = -2\omega \cot \alpha . \dot{x} - \frac{\omega^2}{\sin^2 \alpha} x,$$

or, introducing the value of κ above,

$$\ddot{x} = -2\kappa \dot{x} - (\omega^2 + \kappa^2) x.$$

This differs from the equation for simple harmonic motion (§ 72) by the term involving κ, whose physical interpretation (when it is multiplied by the mass of the oscillating particle) is a *resistance* proportional to the speed of the motion. But the preceding investigation shows us that an equation of this form represents the resolved part (in some definite direction) of uniform circular motion with angular velocity ω, the radius of the circle shrinking in each second by the fraction κ of its amount. (This is the same thing as saying that its logarithm diminishes by κ in unit of time.) Or we may call it simple harmonic motion whose *scale* is constantly diminishing at a definite rate.

This special case of motion is fully described by the equation

$$x = a\epsilon^{-\kappa t}\cos(\omega t + \alpha).$$

Compare the result of § 72.

75. The step to three-dimensional space is now easy. We will take it from a somewhat altered point of view. Our reference system is now three planes at right angles to one another; say the floor, the north wall, and the west wall of a room, the corner in which these three meet being for the time our origin.

And the position of a point is determined without ambiguity if we know its distances from these planes, with the proper sign of each.

For, knowing only its distance from the floor, we limit it to the horizontal plane which is everywhere at that distance from the floor. Similarly the second condition limits it to a definite plane parallel to the north wall. These two conditions together limit it to a certain horizontal line lying east and west. The third condition limits it to a certain plane parallel to the west wall; and this is intersected in one point, and one only, by the east and west line just mentioned. That one is the sole point which satisfies all three conditions.

Thus, let O represent the origin, yOz the north wall,

zOx the west wall, and xOy the floor. The figure is drawn as seen by an eye equidistant from these three planes, and *in the room*, *i.e.* on the positive side of each of them. And it will be noticed that the lettering, x, y, z of the ends of the edges, which meet in O, is so applied that rotation from Ox to Oy, from Oy to Oz, and from Oz to Ox again, will all alike appear to be *counterclockwise.*

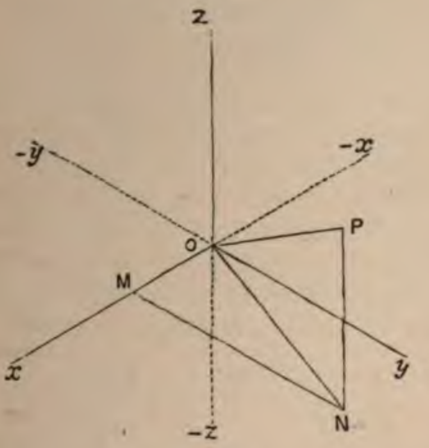

FIG. 6.

The position of any point, P, is then found thus:—Draw PN perpendicular to the floor, meeting it in N; thence NM perpendicular to Ox. Then $OM = x$ is the distance of P from the north wall; $MN = y$ is its distance from the west wall; and $NP = z$ is its distance from the floor.

If P assume a new position which requires it to pass through any *one* of these planes, the corresponding co-ordinate changes sign; if it pass through an edge (*i.e.* the intersection of two of these planes) two co-ordinates change sign; and if it pass through O (where the three planes meet) all three co-ordinates become negative. This is illustrated by the negative lettering at the (dotted) prolongation of each edge through O.

76. But, in analogy with the second method of § 69, we see that the position of P will be fully specified if we know the vertical plane through O in which it lies (*i.e.* the plane zON), the angle NOP in that plane, and the length of OP. The first is determined if we know the angle xON. Hence we determine P by its distance from O, and two angles which (together) enable us to assign the *direction* of OP. The angle xON is called the *Azimuth* of the plane zON; let us denote it by θ. The angle NOP is called the *Altitude* of P, as seen from O; let us denote it by ϕ. Also let the length of OP be, as before, called r.

Comparing, as before, the results of the two methods, we see that $\mathrm{ON} = r \cos \phi$, and therefore

$$\begin{aligned} x &= \mathrm{OM} = \mathrm{ON} \cos \theta = r \cos \phi \cos \theta, \\ y &= \mathrm{MN} = \mathrm{ON} \sin \theta = r \cos \phi \sin \theta, \\ z &= \mathrm{NP} \qquad\qquad\quad\;\, = r \sin \phi. \end{aligned}$$

The elements of spherical trigonometry show that the multipliers of r, in the values of x, y, z respectively, are the cosines of the angles between the line OP and the lines Ox, Oy, Oz. Hence the more symmetrical method, in which these cosines are represented by l, m, n respectively, gives

$$x = rl, \; y = rm, \; z = rn,$$

with the condition

$$l^2 + m^2 + n^2 = 1.$$

It is easy to see that the remark in § 69, as to resolution of a velocity in two dimensions, holds with respect to three.

Then Newton's *Second Law of Motion* (Chap. VI.) at once extends these conclusions to Forces.

77. A remark of great importance must be made here. We saw in § 68 that a point was determined, in x, y coördinates (*i.e.* plane space of two dimensions), as the intersection of two straight lines, to one of which it was confined by its x being given in value, to the other by the value of its y. But *any* two independent conditions connecting x and y will, also, determine their values. A single condition connecting x and y is known as the *Equation of a Curve*, and, when given, limits the position of P to that curve. Two such conditions, therefore, give P by the intersection of two curves, on each of which it must lie. Such a condition applied to a physical particle is called a *Degree of Constraint*. In two-dimensional space a free particle has but two *Degrees of Freedom*, one of which is removed by each degree of constraint to which it is subjected.

78. Similarly we saw that, in three dimensions, the point given by x, y, z is determined as the intersection of three planes, on each of which it must lie. But any one condition connecting the values of x, y, and z is the *Equation of a Surface*, and, when it is given, a particle at the point is subjected to one degree of constraint. When free, it has but three degrees of freedom; and thus three degrees of constraint, by completely determining its x, y, and z, fix its position.

We should arrive at the same result by considering relations among the r, θ, ϕ co-ordinates. But it suffices to consider merely what species of constraint each of these imposes when its value is given. All points for which r has a given value lie on a *sphere* whose centre is at O. When θ is given, the point must lie somewhere in the vertical *plane* zON. When ϕ is given, it must lie somewhere on a right *cone* of which O is the vertex and Oz the axis.

[The two latter statements are easily illustrated by means of a telescope, mounted (in the common way) on a stand which allows it to rotate either about a horizontal, or about a vertical, axis. Place it in any azimuth, and vary its altitude, it turns in a vertical plane about the horizontal axis. Place it at any altitude, and vary its azimuth, it rotates conically about the vertical axis. Hence, by means of these co-ordinates, or conditions, each definite point in its axis is constrained to lie on a sphere, a plane, and a cone, simultaneously.]

79. Two devices are in common use for enabling us to represent, on a plane (or other space of two dimensions) the third dimension.

Thus, in an Admiralty chart, we find the sea-area marked over with figures denoting *Soundings*:—*i.e.* the average *depth* of the water at certain places is written in in fathoms. These soundings are of course more numerous in regions where there are shoals or intricate channels. But it is obvious that, if they were numerous enough, they would enable us to construct a model of the sea-bottom. The soundings, therefore, supply, as it were, the necessary third dimension. But this process, though usually sufficient for purposes of navigation, is at best a rude and incomplete one.

The other method, however, rises to a very high order of scientific importance, not merely from the point of view for which it was originally devised, but on account of the extent to which its essential principles are now applied throughout the whole range of physics. We therefore devote some space to its full explanation.

80. This is called the method of *Contour Lines*, and is employed with great effect in the best maps, such as those of the Ordnance Survey.

A contour line passes through all places which are at the same height above the sea-level.

Thus the sea-margin is itself the contour line of *no* elevation. Suppose the water to rise one foot (vertically). There would be a new sea-margin, in general encroaching more on the land than the former; encroaching most at places where the beach has the gentlest slope, not encroaching at all on a perpendicular cliff, and thrust out (seawards) from an overhanging cliff. This is the contour line of one foot elevation. It is clear that by supposing a gradual rise of the sea, or subsidence of the land, foot by foot, we could obtain a series of curves (each in its turn a sea-margin) gradually circumscribing the uncovered portion of the land, and finally closing in over its highest peak. We require no such natural convulsion as that just imagined. Cloud strata, or fog-banks, with definite horizontal surfaces, constantly show us these appearances in hilly countries. But it is a simple matter of *Levelling* to trace out contour lines, and to draw them on a map of the district. For practical purposes it is usually sufficient to draw them for every 50 or 100 feet of additional elevation above the sea-level.

The celebrated *Parallel Roads of Glen Roy* are merely contour lines, etched on the sides of the valley by long-continued but slight agitation of the margin of the water which filled the glen to various depths in succession, as the barriers which dammed it up were, at intervals, broken down.

Referring to § 78, we see that a surface can be fully described in terms of *one* relation among x, y, and z. Let the plane of Ox, Oy, be that of the sea-level, and let the relation expressing the surface of the land be

$$f(x, y, z)=0.$$

Then the contour lines, as traced on the (two-dimension) map are the curves

$$\begin{aligned}&f(x, y, 0)=0,\\&f(x, y, 100)=0,\\&f(x, y, 200)=0, \text{ etc.}\end{aligned}$$

81. To familiarise the student with the general appearance of contour lines, and their relation to the form of the corresponding surface, we give those of a right cone whose

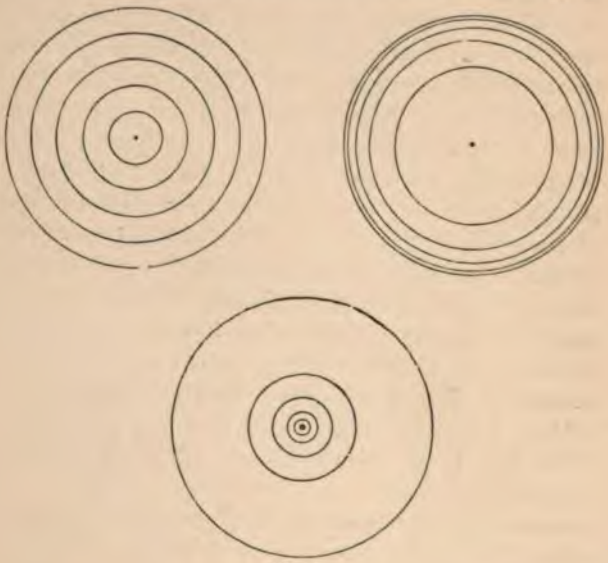

FIG. 7.

axis is vertical, of a hemisphere, and of a fusiform or spindle-shaped body.

The fusiform body, whose contour lines are drawn, is formed by the rotation of a quadrant of a circle about a vertical tangent, the point of contact being the apex. And the contour lines are drawn, in each case, at successive heights increasing by one-fifth of the whole height of the figure. Thus the distances between successive contours, in the two last figures, form the same series of values, but in opposite order.

The equality of distance between the successive contour lines of the cone indicates uniform steepness throughout. In the hemisphere the lines are closer together near the boundary of the figure, in the spindle they close in on one another towards the centre; the hemisphere being steepest at its edges, and the spindle surface steeper towards the point.

82. In fact, the *Gradient* of a surface in any direction (*i.e.* the amount of rise per horizontal foot) is obviously, at any point, *inversely* as the distance in that direction between successive contour lines, for they are traced at successive equal differences of level; and thus the distance between them, along any line drawn on the map, is the space by which we must advance horizontally along that line while ascending or descending vertically through 100 feet.

83. The *line of steepest slope* at any point of a surface is, of course, perpendicular to each contour line where it meets it. For the contour line is horizontal, *i.e.* has no slope. And in the projection on a horizontal plane this perpendicularity remains. Thus the line of greatest slope at any point is represented on the map by the shortest line which can be drawn from that point to the nearest

contour line. It is the path along which a drop of water would trickle down. It is therefore called a *Stream-line.*

84. If the surface be like that of a saddle (concave upwards along the horse's back, convex upwards across it), we have at the middle of the saddle what is called, in geography, a *Col* or mountain-pass:—the lowest point of the ridge between two neighbouring summits. The characteristic of the col is that, at such a point, a contour line *intersects* itself. The following sketch shows the general form of the contours near such a point.

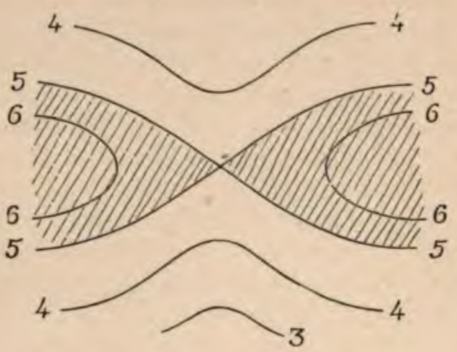

FIG. 8.

In the shaded regions depicted to the right and left of the col the ground rises, in the unshaded regions depicted above and below it falls. [The figures on the contour lines show their order of altitude above the sea-level.]

Other very special peculiarities might be mentioned, but they are not necessary for the beginner; and the more mathematical reader can easily work them out for himself.[1]

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[1] See Cayley, *Phil. Mag.*, XVIII. 264 (1859); Clerk-Maxwell, *Ibid.*, December 1870.

85. If we draw, by the help of the contour lines, the stream-lines (which, § 83, cut them at right angles), we find that they have the following property. In regions *above* the level of a col, they fall away on both sides from that particular one of their number which passes from a mountain *Summit* down to the col, and thence up to the neighbouring summit. This particular line, then, is the *Watershed*, separating two valleys or drainage areas. If we follow the course of the stream-lines into regions *below* the col, we find that they usually approach to the special stream-lines drawn downwards from the col on opposite sides. These will therefore be fed by all the little rills in succession, and thus they become the *Watercourses*. A watercourse is thus the stream-line drawn from a col so as to pass through an *Imit*, or lowest point of the surface.

If we were to take a cast from a model of a surface (with its contour lines) and treat it as a model of another surface, contour lines would remain contour lines, and stream-lines stream-lines; but summits would become imits, and imits summits, while watercourses would become watersheds, and conversely.

86. So far, we have been dealing with contour lines in the ordinary sense of the word. But essentially the same *sort* of thing is presented by the meteorological curves called *Isobars*, and by *Isothermals*, *Lines of Equal Magnetic Variation*, of *Equal Dip*, etc. etc. In each case the lines are drawn, on a two-dimension map, so as to pass through all places where the barometer, or the thermometer, stands at a given reading or level, where the compass deviates a given amount from true north, etc. etc. Thus they have a characteristic similar to that of contour lines, viz. that all points on any one line possess some definite property to exactly the same amount. These applications of the

principle are of great importance, but they do not belong so immediately to our subject as do others, of which we will now give an example or two.

87. Just as water trickles from places at higher, to others at lower, *level*, and as heat flows, in a conducting body, from places of higher, to others of lower, *temperature*, so electricity is said to flow from places of higher, to places of lower, *potential*. Hence, to study the flow of electricity in a sheet of metal, we require to know the lines of equal potential.

The first investigation of this subject, by Kirchhoff,[1] supplies an exceedingly simple and beautiful example.

Putting the wires attached to the ends of a galvanic battery into contact with a very large sheet of uniform tinfoil, at points A and B, (Fig. 9) we establish and maintain a definite difference of potential between those points of the sheet. Hence there is a steady flow of electricity from the one to the other; and it must take place, at every point, in a direction perpendicular to the *equipotential* line passing through that point. Thus, to find the lines of flow of electricity, we must have a means of, as it were, contouring the plate electrically, and finding its lines of equal potential. This is furnished by a galvanometer, for that instrument indicates at once any current passing through its coil of wire. But, if the ends of its coil be kept at equal potentials, no current will pass. Hence, if we put one end of the galvanometer coil in contact with the tinfoil at any point, P, and move the other end about on the foil until no current passes, the point, Q, with which it is then in contact, is at the same potential as P. By fishing about, therefore, we can, point by point, trace out the equipotential line PQ

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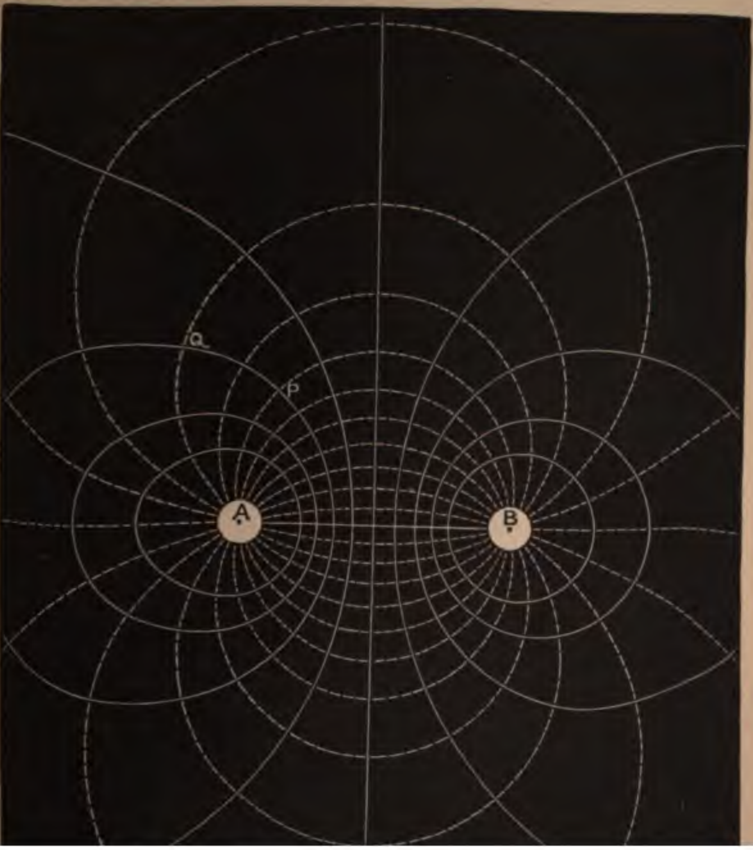
[1] *Pogg. Ann.*, 1845, lxiv.


Q
P
A
B

passing through P. And the same may be done for other points, till we have covered the tinfoil with as many lines of this kind as we desire.

In the special case which we have taken, it was found that when the plate of tinfoil is very large in comparison with the length AB, these lines are circles, whose centres lie in the line AB, and in each of which the ratio BQ/AQ is the same throughout (see § 141); though of course its values are different for different circles of the series. A few of these circles are given (in full lines) in the figure. [To ensure proper contact with the battery, little *circular* discs of copper (indicated in white) are attached to the tinfoil at A and B. The edges of these (on account of the superior conductivity of the copper) are equipotential lines. The points A and B are not exactly at the centres of these discs.]

Now geometry tells us that the lines, which cut at right angles all circles drawn according to the above law, belong to another series of circles:—viz. those which are determined by the condition that each passes through the two points A and B. These circles (some of which are represented by the dotted lines in the figure) are therefore the current lines along which the electricity passes in the tinfoil.

The full circles are drawn for successive equal changes of potential; and the dotted circles which are drawn are so selected that the amount of electricity which flows in a given time through the space bounded by portions of each contiguous pair is the same. If the full lines be regarded as contour lines of a surface, and if A be connected with the positive pole of the battery, the left hand side of the figure represents a hill, and the right hand an exactly equal and similar hollow; so that the halves, as separated by the single straight contour line, would exactly *fit into* one another if the whole could be folded along that line. [This illustrates the last paragraph of § 85.]

If both A and B be connected with the positive pole of the battery, and its negative pole be connected with a massive ring of copper, or other good conductor, which borders the sheet of tinfoil all round at a very great distance from A and B, the equipotential lines are what mathematicians call *Cassini's Ovals*. One of them is the *Lemniscate* of Bernoulli, and its double point corresponds to a col. The figure resembles in general form that of § 84, and the current lines are a series of rectangular hyperbolas.

88. As a final example, somewhat more pertinent to our present work, take the relation between the pressure, volume, and absolute temperature, of a given mass of air.

Experiment has proved that when any two of these three quantities are given, the third is determined. Calling them p, v, and t respectively, the relation between them is (nearly enough for our present purpose) found to be represented by the expression

$$pv = Rt. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where R is a known constant quantity. [In a later chapter we will study the precise relation. What we seek at present is an illustration of *method*, not a specially exact representation of *fact*.]

Now we may treat p, v, and t just as we treated x, y, z in § 80 above. In this statement lies the essence of the value of the contour-line idea as applied to questions of general physics.

Thus the experimental relation among p, v, t, (1) above, may be looked on as the equation of a surface. Let us draw its contour lines on the plane in which p and v are measured.

Equation (1) shows that these lines (three of which are marked in the figure with their temperatures t_1, t_2, t_3,) are all rectangular hyperbolas, of which the asymptotes are the axes of volume and pressure, Ov and Op. Any line of equal pressure $Av_1v_2v_3$ is divided by them so that Av_1, Av_2, Av_3, etc., are proportional to the absolute temperatures. So with a line of equal volume $Bp_1\,p_2p_3$. And one special advantage of this mode of representation is, that the *work* required to compress the gas at any constant temperature, as t_1, from volume OB′ to volume OB, is given by the *area* $B'p'_1\,p_1B$, which is contained between the curve t_1, the axis of volume, and the lines of equal

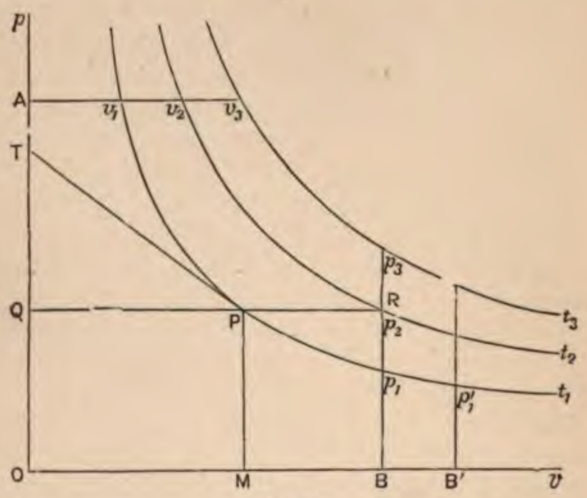


FIG. 10.

volume $B'p'_1$, Bp_1. This follows at once from the fact that the work done during an elementary change of volume *dv*, under pressure *p*, is represented by *pdv*; a little element of area bounded by the curve, the axis of *v*, and two contiguous ordinates.

Draw a tangent PT to one of these curves at a point P, and draw PQ parallel to O*v*. The *compressibility* of a gas, at constant temperature, is the fractional change of volume per unit increase of pressure. It is therefore represented by

$$\frac{\mathrm{QP}}{\mathrm{QT}} \cdot \frac{1}{\mathrm{OM}}, \text{ or } \frac{1}{\mathrm{QT}},$$

or (by a property of the hyperbola) $\frac{1}{\mathrm{PM}}$,

i.e. it is inversely as the pressure.

The *expansibility*, at constant pressure, is found similarly by producing QP to cut the proximate curve t_2 in R; for it is expressed by the fractional change of volume per unit rise of temperature, that is

$$\frac{\mathrm{PR}}{\mathrm{OM}} \cdot \frac{1}{t_2-t_1}, \text{ or } \frac{(t_2-t_1)\,\mathrm{OM}}{t_1\,\mathrm{OM}} \cdot \frac{1}{t_2-t_1} = \frac{1}{t_1}.$$

This is a mere portion of what is called, in *Thermodynamics*, Watt's *Diagram of Energy*, the whole of which is an application of the contour idea.

89. We must now, as promised in § 59, say a few words as to a (possible ?) fourth dimension of space.

Let us treat this from the point of view of what we may imagine would be the experience of beings, endowed with something corresponding to human reason and human senses, but inhabiting space of one or of two dimensions.

In one-dimensional space the inhabitants can have *length* only, and have absolutely no hint from experience of what another dimension could be. Yet we might imagine them, if they were not mere points, to experience some perfectly novel, and to them unaccountable, sensation in passing from part to part of their space *if its curvature were not everywhere the same.* Suppose, for instance, their space to be a line with knots on it.

Similarly, an inhabitant of two-dimensional space (a *bookworm*, as Sylvester once called him) might, even if his dimensions were both finite, pass from place to place in a plane or a spherical space without feeling any new sensation. But, if a part of his space were creased or folded, he might be imagined to feel some strange sensation while he passed through such a part. This is a question of surface-curvature, which would be totally unintelligible to a being whose experience (limited to two dimensions) had not prepared him for it.

So if there should be a fourth dimension, our three-dimensional space may appear to a four-dimensional observer to have something analogous to curvature or creasing: and if, in the course of our solar system's rapid progress through space, we should come to a region of that kind, we may fancy that some absolutely novel form of experience would be the result.

60. Speculations of this nature, however, though based to a certain extent on scientific facts, necessarily involve the question of *sensation* or *perception*; and, in so far as they do so, they pass from the domain of physical science into the realms of Physiology.

CHAPTER V.

IMPENETRABILITY, POROSITY, DIVISIBILITY.

91. Our working definition of matter (§ 21) involves another property besides those discussed in last chapter—viz. *Impenetrability*. The sense in which we are to understand this term depends upon the use of the word *occupy* as applied to space.

On the theory of ultimate atoms, whether the old hard atom (§ 23) or the vortex atom (§ 27), the occupation of space is complete so far as each atom is concerned. Where one atom is, it fills space to the absolute exclusion of every other. But space is not *continuously* filled by the atoms of any portion of tangible matter (§ 24); hence there may be mixtures of atoms of different kinds, which will be the more perfect and intimate the smaller we suppose the individual atoms to be. But there is no use in discussing questions of this kind, at least until we prove the existence of atoms. Thus the strictly scientific use of the term impenetrability need not occupy us.

92. There is, however, a semi-scientific use of the word which is of some importance. For, whether matter be impenetrable in the strict sense or not, we may usefully discuss the consequences of its *not being penetrated*. Thus the hollow of a mould, and *only* the hollow, is

accessible to the liquid metal poured into it. Otherwise "casting" would be impossible.

One of the most important of these consequences was long ago given by Archimedes,—viz. a mode of easily comparing the volumes of bodies of shapes so irregular or complex as to defy the powers of calculators working from mere linear measurements. All that is required is to immerse them successively in a vessel partly filled with water, and to note the amount by which the level of the water is disturbed, *i.e.* (in the usual phrase) the amount of water displaced. Bodies which, when thus tried, displace equal amounts of water have equal volumes, however different may be their figures.

93. Hence, to measure the volume of an irregularly-shaped body:—a lump of stone or coal, for instance:—grease it or varnish it all over, *to prevent water from entering its pores*, *i.e.* to secure non-penetration; and immerse it completely in a vessel partly filled with water. Mark the height to which the water-level rises. Withdraw the stone, and pour in mercury until the same disturbance of water-level is again produced. The volume of the mercury is the same as that of the stone. The mercury has the advantage of taking at once the form of any vessel in which it may be placed, so that its volume may be promptly determined by pouring it into a properly graduated beaker.

This simple consideration forms one of the bases of the common method of measuring specific gravity (§ 36) by weighing a body, first in air, and then when it is suspended in water.

94. But it is not solids (such as the stone above) or other liquids (such as mercury) alone which can thus displace their own bulk of water. Air will do equally

well. Thus a diving bell is merely an open-mouthed vessel, inverted and let down into water. The air it contains is not penetrated by the water, and thus displaces (just as a solid or a liquid would have done) its own volume of water. Its volume, no doubt, becomes less as the bell descends under the water, but this is due to the increase of hydrostatic pressure to which it is subjected. Still, however it be compressed, as it is not penetrated it displaces at every instant its bulk of water.

95. When one liquid mixes with, or when it combines with another, it does not usually displace its own bulk of the other. In such cases there is interpenetration.

Thus, when twenty-seven parts (by weight) of water are mixed with twenty-three of alcohol, the volume of the mixture is less by 3·6 per cent. than the sum of the volumes of the constituents.

When an alloy of tin and copper, such as used to be employed for the specula of large reflecting telescopes, is formed, the joint bulk may be as much as 7 or 8 per cent. less than the sum of the bulks of the constituents.

And Faraday showed that, when potassium is oxidised, the resulting potash has a less volume than *either* of the constituents.

96. But, as a rule, in these cases of contraction, other physical phenomena present themselves. Thus the mixture of alcohol and water above described becomes more than 8° C. warmer than the components, if both were taken at the same (ordinary) temperature. A rod of tin dipped into melted copper (at a very dull red heat) produces vivid incandescence as it melts and is alloyed. And the combination of oxygen and potassium develops kinetic energy at an almost explosive rate.

There are other cases, which we need not treat of here (especially as they belong properly to *Heat* and to *Chemistry*), in which the volume of a mixture is *greater* than the sum of the volumes of its constituents.

97. These examples show that Archimedes' notable process might altogether have failed in its application. For he is said to have been asked to find whether the votive crown made for Hiero of Syracuse really consisted of the amount of gold furnished for its manufacture, or whether a part of the gold had been abstracted, and its place supplied by an equal weight of silver.

He procured lumps of silver and of gold, each equal in weight to the crown. These he immersed successively in a vessel filled to the brim with water, measuring in each case the amount of overflow, which he found to be greater for silver than for gold. The vessel being once more filled, the crown itself was immersed, and was found to displace more water than did the gold. Hence, by calculation, Archimedes found how much silver had been substituted for an equal weight of gold.[1] This calculation, of course, must have proceeded on the supposition that the bulk of the alloy was *equal* to the sum of the bulks of the component metals.

But interpenetration, of which he had no knowledge, might have completely baffled the great mathematician.

If a similar question were raised *now*, it would of course be decided at once by the processes of the chemist, not by those of the physicist.

98. We have seen (§ 24) that, on the hypothesis of hard atoms, there must necessarily be interstices between them, else bodies could not be compressible.

But it is an experimental fact, independent of all hypotheses, that bodies in general are *Porous*. By the term pores we do not refer to *visible* channels, such as those which run in all directions through a piece of sponge, but to microscopic channels, which pervade even the most seemingly homogenous and continuous substances, such as solid lead, silver, gold, etc.

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[1] The original passage is given as *Appendix III.*

The proof that such channels exist was given experimentally by Bacon, who tried to compress water by squeezing or hammering a leaden shell filled with water and closed. The water exuded like perspiration through the pores of the lead. The Florentine Academicians tried the same experiment with a silver shell, but obtained the same result. They then tried to prevent the escape of the water by thickly gilding the shell, but again in vain.

99. When a corner of a piece of blotting-paper, or of a lump of loaf-sugar, is dipped in water, we see (especially if the water be coloured) the rapid entrance it effects into the pores. *Why* it enters, under these conditions, is another question. (Chap. XII.)

The porosity of wood, necessary for the circulation of the sap, is beautifully shown by the fact that, from microscopic examination of a thin slice of a fossil tree, a botanist can tell at once the species to which it belonged. The greater part of the *material* of the wood has disappeared for it may be millions of years, but its microscopic *structure* has been preserved by the infiltration of silicious or calcareous materials which, hardening in the pores, have thus preserved a perfect copy of the original. The rapid passage of gases through unglazed pottery, iron and (hot) steel, etc., shows the porosity of these bodies in a very remarkable manner. So does the strange absorption of hydrogen by a mass of palladium. (See Chapter XIII.) The porosity of steel has recently been shown in a most remarkable manner by Amagat, who forced mercury through a thickness of more than 3 inches under a pressure of at least 4000 atmospheres. (§ 196.) The metal was quite impervious to glycerine under the same pressure.[1]

Another beautiful instance is afforded by the silicious concretion, *Tabasheer*, found in the joints of sugar-canes. It is opaque when dry, but when immersed in water for a short time becomes transparent. Certain agates, called *Hydrophanes*, exhibit the same property.

100. No decisive proof of the porosity of vitreous bodies, such as glass, seems yet to have been obtained. That they form almost a solitary class of exceptions to an otherwise general rule seems highly improbable. And instances, such as those given below, seem to indicate that these vitreous bodies have not yet been proved to be porous solely because we have not discovered the proper mode of testing them.

When polished marble is wetted with water, very little enters the pores; while oil, on the contrary, is rapidly absorbed.

A bag of cambric or gauze, the holes in which are visible to the eye, holds mercury securely, until sufficient pressure is applied to force out the liquid. (§ 288.)

Glazed pottery-ware, which is practically impervious to hydrogen and to pure water, is rapidly penetrated by a strong aqueous solution of bichromate of potash. This solution, crystallising in the pores, disintegrates the whole, just as water, freezing in the pores of a rock, gradually breaks its surface-layers into small fragments, to be afterwards washed down to the plain as alluvial soil.

The question of the porosity of colloidal bodies, such as gelatine, albumen, and, from some points of view, india-rubber, is somewhat puzzling. We will refer to it in Chapter XIII.

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[1] *Comptes Rendus*, March 2, 1885.

101. The *Divisibility* of matter, in the strict scientific sense, at once raises the question of the existence of finite atoms. For, if there be such atoms, division has them for its limit, whatever processes may be employed.[1] We are not prepared to face this aspect of the question, and must, therefore, confine ourselves to examples of extremely minute *Division*.

An "impalpable" powder is one which gives no gritty sensation when we rub it between the thumb and fingers. The process of *Levigation*, depending on fluid friction (§ 38), is employed for the assortment of solid particles into packets of different degrees of fineness. Thus, if ground emery be thrown into a tall vessel full of water, we may remove from the bottom of the vessel successive crops, as it were, of gradually increasing fineness. Yet even the finest of these powders can be used for grinding metallic or glass surfaces, showing that its particles still possess the same properties as do those of the coarser powders.

Silica may be thrown down, by chemical processes, in such an extreme state of division that when it is dried and poured into a trough it behaves almost like a liquid. Especially when it is heated, we observe that, like a liquid, it is capable of propagating gravitation-waves. Calcined magnesia and other very fine powders show similar properties.

102. Even the rough process of scratching the polished surface of glass with a diamond point can be carried out by machinery to such an extent of delicacy that groups of equidistant parallel lines may be traced, some of which

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[1] See again *Appendix II.*

can only be "resolved" into their components by the very best microscopes; others, which we have every reason to believe capable of resolution, have not yet been resolved. These pieces of ruled glass are known to microscopists as *Nobert's Test*.

Ordinary gold-leaf, though prepared by a very rough process, has a thickness of about 1/300,000th of an inch only. But, as Faraday showed, it can be rendered very much thinner by immersion in a solvent such as cyanide of potassium. And, by a species of inversion of Wollaston's process (§ 49), *i.e.* drawing into very fine wire a silver rod thickly gilt, we obtain a continuous film of gold, whose thickness is estimated at less than 1/4,000,000th of an inch.

103. The average size of the particles of water in a light cloud is easily estimated from the diameter of the coloured rings, or *Coronæ*, which it produces when it covers the sun or moon.[1] If the radius of the innermost ring be 15°, the diameter of the particles must be about 1/13,000th of an inch. Such must have been the average size of the dust particles from the Krakatao eruption of 1883, which produced the remarkable sunsets, as well as the corona seen about the sun when no cloud was visible. The length of time these particles remained in suspension is accounted for in § 40 above.

104. Leslie, in his *Natural Philosophy* (1823), says: "A single grain of musk has been known to perfume a large room for the space of twenty years. Consider how often, during that time, the air of the apartment must have been renewed and have become charged with fresh odour! At the lowest computation the musk had been subdivided into 320 quadrillions of particles, each of them capable of affecting the olfactory organs." Leslie does not

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[1] Tait's *Light*, 2nd ed., §§ 180, 246.

tell us how the computation was made, nor even what we are to understand by quadrillions.

[The usual British reckoning gives a quadrillion as a billion billions, each billion being a million millions, while the French reckoning makes it only a thousand million millions. This confusion is entirely removed by the modern mode of writing large numbers, which we know only in rough approximation. We write the first two or three significant figures, and indicate the number of the remaining ones by the corresponding power of 10.]

Thus Leslie may have meant either 320×10^{24}, or 320×10^{15}. If, as is most probable, he meant the former of these numbers, the result of his computation has been singularly verified by recent discoveries, some of them apparently altogether unconnected with the question before us.

105. One of the most striking instances of division is that furnished by holding, in an otherwise slightly luminous flame, a particle of common salt or of some other metallic chloride. Fox Talbot, in 1826, wrote: "A particle of muriate of lime on the wick of a spirit-lamp will produce a quantity of red and green rays for a whole evening without being itself sensibly diminished." Swan traced the source of the peculiar orange ray which appears in the light of almost every flame to the wide diffusion of exceedingly small quantities of common salt. These phenomena are nowadays known to all in connection with *Spectrum Analysis*. The quantity of common salt which, for a considerable time, will continue to give the orange tinge to the flame of a large Bunsen lamp is minute in the extreme. The effect is now proved to be due to *vapour* of sodium.

106. A conviction of the practically infinite divisibility of matter must be felt by all who would hold a warranted belief in the "dilutions" which are at least popularly supposed to be one of the main characteristics of homœopathic medicines. When a single grain of atropine is dissolved in a gallon of water, and *one drop* of this is added to another gallon of water, we have what is called the *first dilution.* Add a drop of this to a third gallon of water, and we have the second dilution. And so on. Tenth dilutions are said to be sometimes administered. If we take the diameter of a drop as about 1/8th of an inch, we find, by an easy calculation, that (as there are 277 cubic inches in a gallon) the tenth dilution should contain about $2/10^{60}$ of a grain of atropine per drop! If that drop were magnified to the size of the whole earth, the atropine in it (magnified, of course, in proportion,) would correspond to a particle of somewhere about one three-billionth of an inch in diameter!! So that, unless atropine can be separated into particles of this excessive smallness, one drop of the so-called dilution will be totally different from another:—one being a potent medicine, the other pure water.

107. The kinetic theory of gases informs us that, in a cubic inch of any gas at atmospheric pressure, and at ordinary temperatures, there are somewhere about 3×10^{20} detached particles absolutely similar and equal to one another. These cannot be Lucretian atoms, for they have each many different modes of vibration, even when they belong to a simple and not to a compound gas. Here we reach the limit of our present knowledge as to division of matter. What is the structure of these gaseous particles on which their vibrations depend (§ 29), and how far further divisible each particle must be

supposed in consequence, are matters beyond our knowledge. [These results of the kinetic gas theory are confirmed by altogether independent lines of physical reasoning with which we are not concerned here.]

We may take, as a rough approximation, that the grained structure (§ 26) of the most nearly homogeneous solid or liquid bodies is of the order of 5×10^8 to the inch linear. To give a notion of the amount of division which this indicates, suppose we magnify a cubic inch of such a substance to a cube whose side is the diameter of the earth. The earth's polar diameter is 5×10^8 inches, very approximately. Thus, in the enormously magnified cube, there is one particle in every cubic inch or so. We say nothing, it is to be noticed, as to the size of the particle or granulation itself. [The estimates hitherto made of this quantity can hardly be called even rough approximations:—so widely do they differ from one another, according to the particular physical phenomenon on which each is based. But probably the particle does not occupy so much as 5 per cent. of its share of the whole content.]

All that can be said of the above estimates (of *number* of particles per cubic inch) is that they are, at least nearly, of the proper *order* of magnitude. And it is curious to find that the result of Leslie's old "computation" (§ 104) agrees fairly enough with our present knowledge.

CHAPTER VI.

INERTIA, MOBILITY, CENTRIFUGAL FORCE.

108. We commence with Newton's

First Law of Motion.

Every body perseveres in its state, of rest or of uniform motion in a straight line, except in so far as it is compelled by forces to change that state.

The property, thus enunciated as belonging to all bodies, is usually called *Inertia*. And it is clear from the statement above that it may be described as passivity, or dogged perseverance, but in no sense whatever as revolutionary activity. This consideration will be found presently to be of great importance.

Matter is, in Newton's system, regarded as the plaything of force; submitting to any change of *state* that may be imposed on it, but rigorously persevering in the state in which it is left, until force again acts upon it.

109. The *state* referred to is one *of rest, or of uniform motion in a straight line* (of which rest is a mere particular case). Here we meet with a serious difficulty.

All translatory motion (including rest, or null motion) is, from the very nature of space, essentially *relative*.

Relatively to *what*, then, are we to say that a body not acted on by force moves uniformly in a straight line? The answer, so far as we can give it, is—

Relatively to any set of lines drawn in a rigid body, of finite dimensions; which is not acted on by force, and which has no rotation.

As will be seen later, (§ 131) Newton has pointed out a physical test, by which it can be ascertained whether a body has rotation or not.

But questions of this kind can only be alluded to, certainly not fully discussed, in an elementary work.

110. The grand proof of the truth of the first as well as of the other *Laws of Motion* is furnished by the celestial motions. So irregular is the motion of the moon, when considered carefully, found to be, that no amount of the most exact observation alone (*i.e.* unaided by physical investigations) could enable us to predict its place, even twenty-four hours beforehand, with anything like the accuracy with which it is predicted four years beforehand in the *Nautical Almanac*. So convinced have astronomers become of the truth of the laws of motion, which are necessarily involved in all their lunar and planetary calculations, that when a discrepancy between prediction and observation is found to occur no one now questions the bases of the calculation. The discrepancy is used to correct our previous estimates of the *elements* of the lunar or planetary orbit; or, as in the notable case of Uranus, it is employed as an indication of where to seek for some undiscovered body whose influence has not been taken into account.

111. Familiar instances of Inertia present themselves in all directions. When a railway carriage is running uniformly on a straight piece of road, we become uncon-

scious of the motion unless we look out at external bodies; but we detect at once any sudden, or even rapid, change of speed. If the motion of the train be checked by a sudden application of the brake, their inertia (which really *maintains* their motion) appears to urge the passengers *forwards*. A sudden starting of the train produces the opposite effect. While the steady motion continues, a conjuror can keep a number of balls in the air just as easily as if the carriage were at rest. But these things need not surprise us. Our rooms are always like perfect railway carriages in respect of their absolutely smooth, but very rapid, motion round the earth's axis. The whole earth itself is flying in its orbit at the rate of a million and a half of miles *per day*; yet we should have known nothing of this motion had our globe been perpetually clouded over like that of Jupiter. The whole solar system is travelling with great speed among the fixed stars, but we know of the fact only from the minutely accurate observations of astronomers, aided by all the resources of the *Theory of Probabilities*.

112. When a bullet is dropped from a definite point in a uniformly running carriage, it strikes the same point of the floor whatever be the speed of the motion; for, by its inertia, it preserves while falling the forward motion of the carriage which it obviously had while it was held in the hand. But, if the bullet be dropped from the yard of a vessel to the deck, it will *not* fall always on the same spot, however uniform be the ship's progress, if there be any *pitching*. For, when the vessel pitches, the yard moves forward alternately faster and slower than does the deck.

Now the top of a tower (unless it be at one of the poles) is farther from the earth's axis than is the foot, or the ground on which the tower is built; and, therefore, as both complete their revolution in twenty-four hours, the top of the tower moves permanently faster than does the base. Hence even a truly spherical bullet, dropped from the top, does not fall vertically. It deviates to the east of the vertical, because it preserves while falling its superior eastward speed. In this way we obtain one physical proof of the earth's rotation.

113. The upsetting of buildings by an earthquake furnishes a striking instance of inertia. So does the almost perfect immunity we experience from the millions of meteoric stones which are constantly encountering the earth with *planetary* velocities. This is due to the inertia of the air, which, in its turn, is one indispensable cause of the destructive action of a tornado; just as, on a smaller scale, a cannon-ball would be harmless without inertia, while an earthwork, without inertia, would afford no defence against it. But we need give no more instances—the reader will easily supply others from his own experience.

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114. So far, we have been speaking of inertia as manifested by the tendency of a body to persevere in its motion with *unaltered speed*. But we must carefully note that this is only one part of Newton's Law. The state in which he tells us that bodies persevere by inertia is not one of uniform motion merely, but of *motion in a straight line*. The preservation of the rectilinear path is quite as essential a part of the functions of inertia as is the preservation of the uniform speed. Hence, just as (following Newton's system) we attribute any change in the speed of a body to the action of force, so, if its line of motion be not straight, (whether the speed be unaltered or no) its curvature also must be due to the action of force.

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115. How the force must be applied which causes a body, in spite of its inertia, to move in a curve is easily understood from some common instances, though it is pretty obvious that it must be in a direction *perpendicular* to that of the motion; and, of course, in the plane in which the curvature takes place. For any force *in* the direction of motion must tend only to increase or to diminish the speed.

It is found that, at a curve on a railway line, it is the *outer* of the two rails which is most worn (*i.e.* that one which forms the convex side of the track). And, when a sharp curve has to be taken rapidly, the outer rail has generally to be laid a little higher than the other. But (except when the brake is on) the pressure is mainly perpendicular to the rails. Hence the force which causes the carriages to move in a curved path must be directed *inwards* to the centre of curvature.

When we whirl a sling with a stone in it, we feel the tension of the cord (which is constantly *pulling* the stone from its natural straight path *in* towards the hand) increased as we cause the sling to rotate faster.

A bullet, suspended by a string, forms what we call a *simple pendulum*. It can, by proper initial projection, be made to revolve uniformly in a *horizontal* circle. It is then what is called a *Conical Pendulum*. Here the tension of the string may be resolved into two parts; one vertical, which supports the weight of the bullet, the other horizontal, which continually deflects the bullet from its natural rectilinear path. If the string could be made long enough, the time of revolution might be made twenty-four hours; and if the pendulum were then set up at the north pole, and made to describe its circle in the positive direction (§ 65), it would appear to remain suspended at rest in the air, the supporting string not being vertical! If it were made to revolve in the negative direction, it would appear to complete a revolution in twelve hours.

The moon is caused to move in an (approximately) circular path about the earth by the same *attraction* which causes stones to fall vertically downwards.

116. One of Newton's remarks on the First Law of Motion runs thus:—

"A hoop, whose parts by their cohesion perpetually draw one another aside from rectilinear motions, does not cease to rotate, except in so far as it is retarded by the air."

Thus the uniformity of the earth's rotation about its axis, which is the basis of our measurement of time, is merely an example of the First Law of Motion.

But when a fly-wheel, or a grindstone, is made to rotate so fast that the cohesion of its parts is no longer capable of supplying the forces requisite to keep them moving in their circular paths, it *bursts* (this is the technical word), and the fragments fly off in paths which are tangential and rectilinear, except in so far as gravity modifies them.

If the rotating body be plastic, as must have been the case long ago with the earth as a whole, its form will be modified by the tendency of every particle to preserve its rectilinear path. Thus it swells out in all directions perpendicular to the axis of rotation. Jupiter and Saturn, being much larger than the earth, and also rotating more rapidly, show this effect in a much greater degree.

A beautiful example is furnished by suspending an endless chain by a cord, and (by very rapidly twisting

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the cord by means of multiplying gear) throwing it into rotation. When the rotation of the whole is sufficiently rapid it assumes almost exactly the form of a *horizontal* circle, all its links being equidistant from the (vertical) axis of rotation.

117. The old notion, probably suggested by such instances as the pull which the stone in a sling seems to exert on the hand, was that bodies have a tendency to *fly outwards* from the centre about which they are revolving. Hence they were said to exert *Centrifugal Force*, and a *Centripetal Force* was of course required to balance this. The term Centrifugal Force has become rooted in our scientific language. It is a convenient enough expression, provided we do not split it up, thus taking it to imply force, and flying from a centre; but interpret it merely as indicating that, to keep a body moving in a curve instead of in its natural straight line, a force directed *towards* the centre of curvature is always required. But, as the third law of motion (§ 128) tells us, a force is only one-half of a stress, so that when force is exerted to pull the body inwards from the tangent, an equal force must be exerted *at the centre* tending outwards from it.

We might quite as justly speak of the *Onward Force* of a cannon-ball, which requires a resistance to check it; as of the centrifugal force (understood not as a single term but as two words, each with its ordinary meaning) which must exist because it requires centripetal force to balance it.

118. Calculating (as in § 71) from the earth's mean equatorial radius, 3962 miles, and the number of seconds in a sidereal day, 86164, we find that the acceleration of a point on the equator is about 0·1116 foot per second,

per second:—while the acceleration due to gravity is about 32, in terms of the same units. Thus about $\frac{1}{289}$th of its weight is required merely to *keep* a body on the earth's surface at the equator. By this amount its weight (as indicated by a *Spring-balance*, § 165) would be diminished.

If the earth had been revolving seventeen times faster than it does, this apparent diminution of weight would have been 17^2 (or 289) times greater than it is, *i.e.* bodies at the equator would have shown no apparent weight, provided they moved along with the same velocity as the ground below them.

119. As is pointed out in the preceding extract from Newton (§ 116), a wheel or other body, rotating about an axis and not acted on by forces, perseveres by inertia in its uniform rate of rotation. But it does more; it preserves (even when acted on by small forces) the *direction* of its axis of rotation, provided at least that it be rotating about its axis of greatest or of least moment of inertia (§ 132). It is for this reason that rifling of the bore of a gun has been introduced; and also that a skilled player, when throwing a quoit, gives it rotation in its own plane.

The rotation of the earth about its axis is a more complex phenomenon, because it takes place under the action of considerable forces which tend to make the earth revolve about axes lying in the plane of its equator. Yet, because the moments of inertia (§ 132) about all such axes are approximately equal, the period of the daily rotation is not altered, though the *direction* (in space) of the polar axis is affected by Precession and by Nutation. We cannot, however, do more than allude to matters of this order of difficulty. They are all beautifully illustrated by means of Gyroscopes, Gyrostats, etc., but the full study of the phenomena requires higher mathematics than we can introduce here. These are properly questions of *Abstract Dynamics.*

120. Newton's Second Law of Motion is as follows:—

Change of momentum is proportional to force, and takes place in the direction in which the force acts.

Thus, according to Newton, a force *always* produces change of momentum. Hence there is no balancing of forces, though there may be balancing of the effects of forces.

Every force (however small) produces its proper change of momentum. This used to be stated, under the name of *Mobility*, as a characteristic property of matter.

This change is always gradual, never abrupt. An infinite force would be required to produce a finite change of momentum abruptly.

As *change* of momentum alone is mentioned, it is clear that Newton means that the effect of a force is independent of the state of motion of the body to which it is applied. Hence if a force be uniform, as for instance is practically the case with the action of gravity upon a falling body, the additional momentum produced by it in each and every second will measure its amount. But if it be variable, we must measure it by the *rate* at which momentum is produced by it instead of the momentum produced by it in one second. Thus the true measure of a force is the rate of change of momentum; or, to use the kinematical term, the product of the mass of a body into the *acceleration* of its velocity.

121. Two special cases, of great importance, must now be treated:—uniform acceleration in the direction of motion, and uniform circular motion.

It is found that, *in vacuo*, all bodies acquire, per second, an additional vertical velocity of about 32·2 feet per second. This quantity (which varies with the latitude, height above sea-level, etc. § 165) is usually denoted by the letter g. Hence, if M be the mass of a body, its weight (*i.e.* the force which accelerates its fall) is measured by the product Mg.

Kinematics (as we saw in § 71) shows us that when a point moves uniformly in a circle, the acceleration is directed inwards to the centre, and its magnitude is the square of the speed multiplied into the curvature of the path. Hence to keep a body, of mass M, moving with uniform speed V in a circle of radius R, a force whose magnitude is MV^2/R, directed towards the centre of the circle, must constantly act upon it. As Mg is the weight (W) of the body, we may express this force as

$$\frac{V^2}{gR}W.$$

If ω be the *Angular Velocity* in the circular path, *i.e.* the angle described in unit of time by the radius drawn to the moving body, we have obviously

$$V = R\omega,$$

and the expression for the requisite force takes the form

$$MR\omega^2, \text{ or } \frac{R\omega^2}{g}W.$$

122. Newton shows that, as an immediate consequence of the Second Law, we have the *Law of Composition of Forces* acting at one point; the so-called *Parallelogram*, or *Triangle*, of Forces. This follows from the facts that (a) the changes of velocity produced in the same time, by different forces acting on the same body, are proportional

to and in the directions of the several forces, and that (*b*) the effect of each force is independent of the simultaneous action of the others. Thus the problem is reduced to the obvious kinematical composition of velocities (§ 69).

123. We are now prepared to measure both *Masses* and *Forces*. But, for this purpose, it is necessary to have units in terms of which to measure.

The British unit of mass is the *Standard Pound*, whose amount was probably adopted in old times for reasons of convenience, but is now fixed by law.

The French, or Metrical, unit of mass is the *Kilogramme*, originally intended to be the mass of a cubic decimètre, or *litre*, of water at its maximum density point; but, practically, defined by a platinum standard. The Kilogramme is about 2·20462 Pounds.

124. The British *Unit of Force* is such that, when it acts for one second on a mass of one pound, it produces in it a speed of one foot per second.

The C. G. S. Unit of Force (or *Dyne*), which is coming into use for scientific measurements, is such that, when it acts for one second on a mass of one gramme, it produces in it a speed of one centimètre per second.

The British Unit of Force is about 13,825 Dynes. Since the speed acquired by a body falling for one second (*in vacuo*) is (§ 120) about 32·2 feet per second, the Weight of a Pound is about 445,165 Dynes.

125. Generally, if a definite force act upon any mass for one second, it will generate in it a speed whose magnitude is inversely as the mass.

Thus the comparison of masses, *i.e.* their measurement in terms of some standard unit, becomes a perfectly definite scientific process.

It may not be easy to carry out, and in fact it is not;

at least by any very direct application of the principles just explained. That, however, is another matter. No one in his senses would question the perfectness of Euclid's process for dividing a straight line into a given number of equal parts, on the ground that it is practically inapplicable when we try to carry it out.

What we have sought is an accurate, and an easily intelligible, method of comparing masses. If it be not easily workable in practice, we must find something more workable:—just as we now use a screw dividing-engine instead of Euclid's unrealisable straight lines, and still more unrealisable parallel straight lines.

126. When we have measured the mass of a body, and also its volume (§ 92), its *average*, or *mean*, *density* follows at once. If the body be homogeneous, this is its actual density throughout:—and whether it be homogeneous or not, its mean density is simply the average amount of mass per unit of volume.

On account, chiefly, of the remarkable result of Newton's (§ 34), we postpone all considerations regarding the densities of various kinds of matter until we are dealing with their *specific gravities* also.

127. The process of weighing is, as Newton showed (§ 34), essentially a comparison of masses. So that our measurement of mass is practically carried on by means of the *Balance*, which is one of the most delicate and accurate instruments of precision yet invented.

The processes for measuring force are not yet nearly so accurate. Numerous instruments have been devised for the measurement of special classes of forces, the great majority depending upon elasticity of matter. Some of the more important of these will be mentioned when we require them; but the reader must be reminded that on

Newton's system the *true measure* of a force is the momentum it produces in one second.

128. The first two laws of motion (applied with sufficient mathematical resources) enable us to solve any problem whatever regarding the motion of a body, treated as a mere particle, under the action of any given forces. Conversely, they enable us, from the given motion of a particle, to find the forces under which the motion has taken place. But they do not suffice for the calculation of the motion of two or more particles which mutually influence one another, whether by gravitation, or cohesion, or by any physical mode of attachment. Hence the necessity for the

THIRD LAW OF MOTION.

To every action there is always an equal and contrary reaction; or, the mutual actions of any two bodies are always equal and oppositely directed.

In modern speech Newton's *first* explanation of the sense in which this statement is to be understood may be simply expressed thus:—

Every action between two bodies is a Stress.

In this sense it is very closely connected with the first law. For a *system* of two bodies, considered as one, cannot set itself in motion. Even when the masses are not connected in any way, the equality of action and reaction involves transference of momentum between them which leaves the motion of their *Centre of Inertia* unaffected. It was by floating on water a magnet and a piece of iron, both attached to the same light board, that Newton proved the equality of action and reaction for magnetic force. He also proved it by showing that, if the gravitation action of one part of the earth on the rest were not exactly reciprocated, the earth (as a whole) would alter its existing state of motion.

129. The term centre of inertia, employed above, requires a few words of explanation. We assume the following proposition, which can be established by very elementary mathematics.

In every group of massive particles there is one definite point, such that, whatever plane be drawn in space, the distance of the point from that plane, multiplied by the sum of the masses, is equal to the sum of the separate products formed by multiplying each mass into its distance from the plane. It can, therefore, be found by executing the requisite calculations for any three planes whatever, provided no two of them are parallel. This is the centre of inertia of the particles.

When the particles are severally acted on by forces, it follows from Newton's Third Law that the centre of inertia of the group moves as if the whole mass were there concentrated, and acted on by all the forces simultaneously. This consideration greatly simplifies kinetical problems connected with a rigid system or a group of particles; for it enables us to commence by determining the motion of the centre of inertia as if it were a mere particle, and afterwards to study the motion *relatively* to that centre.

130. But Newton proceeds to point out that there is a *second* sense in which the terms *action and reaction* in the Third Law may be interpreted, the law itself still remaining true. In modern phrase it may be expressed as

The activity of an agent (or the rate at which it does Work), is equal to the counter-activity of the resistance.

Newton's statement of this second mode of interpret-ing the Third Law has been shown to require comparatively little addition to make it a complete enunciation of the *Conservation of Energy* (§ 7).

131. If two masses be connected by a spiral spring, but be otherwise free, and if the spring *remains* stretched to a constant amount, this can only be because the bodies are revolving about one another. For, otherwise, the stress in the spring would have caused them to approach one another. This is Newton's test of the existence of *Absolute Rotation*, § 109. For, by the first law, the stress which certainly acts on the masses must interfere with their *states*, and by the third law it must do so in opposite directions. Each, therefore, must be describing a *curved* path relatively to the other, and this must of course be circular.

Nothing is known, nor is anything conceivable even by the most transcendental of metaphysicians, which could give us an indication of *Absolute Translation*.

132. We conclude the chapter with a few additional illustrations and explanations connected with inertia.

In § 119 above we introduced, without explanation, the important term *Moment of Inertia*. This quantity is defined, for any body, with reference to any assigned axis. It is the sum of the products obtained by multiplying the mass of each small portion of the body into the square of its distance from the axis. Its use is twofold.

(*a*) If ω be the angular velocity of a rigid body about an axis, r the distance of the particle whose mass is m from that axis, the speed of m is $r\omega$, and the kinetic energy of rotation (half the product of each part of the mass into the square of its speed) is

$$\tfrac{1}{2}\Sigma(mr^2).\ \omega^2 = \tfrac{1}{2}\mathrm{I}\omega^2,$$

half the product of the moment of inertia into the square of the angular velocity.

(β) Again, the *Moment of the momentum* of a particle about an axis is defined as the product of its momentum by the shortest distance between the axis and the line of motion of the particle. Hence the moment of momentum of m about the axis is $mr\omega.r$, and the whole moment of momentum of the body is

$$\Sigma(mr^2).\omega = \mathrm{I}\omega,$$

the product of the moment of inertia into the angular velocity.

133. It is shown in treatises on Dynamics that the effect of a pair of equal and opposite forces, whose lines of action are different (called by Poinsot a *Couple*) is to produce moment of momentum in proportion to the time it acts and to the moment of the couple. Hence, if Q be the (constant) moment of the couple, ω the angular velocity it produces in time t, when its plane is perpendicular to the axis above spoken of,

$$\mathrm{I}\omega = \Sigma(mr^2)\omega = \mathrm{Q}t, \quad . \quad . \quad . \quad . \quad (1)$$

whence

$$\tfrac{1}{2}\mathrm{I}\omega^2 = \tfrac{1}{2}\Sigma(mr^2)\omega^2 = \mathrm{Q}.\frac{\omega t}{2} = \mathrm{Q}a, \quad . \quad . \quad . \quad (2)$$

where a is the angle through which the body has turned. For ω grows uniformly, and therefore its *average* value during the time t was $\omega/2$, so that the whole *angle* described is $\omega t/2$.

But if a (constant) force P act on a particle, of mass M, and produce in time t a speed v, we have

$$\mathrm{M}v = \mathrm{P}t. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The speed increases uniformly, so that its average value is $v/2$, and therefore the *space* described is $s = vt/2$. Hence, by multiplying both sides by $v/2$, we get

$$\tfrac{1}{2}Mv^2 = Ps. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

It is obvious that in the former pair of equations, (1) and (2), the quantities I and Q, ω and α, play exactly the same parts as do M and P, v and s, respectively, in the latter pair, (3) and (4).

This analogy shows, at least in part, the great convenience of the idea of the moment of inertia.

For special purposes we often write I in the form Mk^2, k being then the common distance from the axis at which every one of the particles must be placed, so that the whole may have the same moment of inertia as before. It is called the *Radius of Gyration.*

134. As an illustration of the application of the two interpretations of the third law, suppose a fly-wheel whose axle (horizontal) is carefully mounted on friction rollers, to be set in rotation by the descent of a weight attached to a string wound 'round the axle.

Let ω be the angular velocity produced in the fly-wheel when a length x of the cord has been unwound, a the radius of the axle, M the mass of the appended weight, I the moment of inertia of the wheel, and T the stress in the cord.

Then the rate of increase of momentum of the mass M is $M\ddot{x}$ (with Newton's notation, § 72). This must be the measure of the force producing it, so that

$$M\ddot{x} = Mg - T \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

The rate of increase of moment of momentum of the fly-wheel is $I\dot{\omega}$, which must measure the couple producing it. Hence

$$I\dot{\omega} = Ta \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

But $a\omega$ is the amount of cord unwound per second, *i.e.* the rate of descent of the weight. Thus

$$a\omega = \dot{x} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3.)$$

(1) and (2) are dynamical equations, (3) is kinematical. x, ω, and T are to be found from the three. They give

$$\ddot{x} = \frac{\text{M}a^2g}{\text{M}a^2 + \text{I}} = \frac{\text{M}a^2g}{\text{M}a^2 + mk^2} \quad . \quad . \quad . \qquad (4.)$$

where m is the mass of the wheel, and k its radius of gyration. (§ 133.)

If the wheel had no moment of inertia this would become

$$\ddot{x} = g,$$

the ordinary equation of acceleration of a free falling body.

Hence, the only effect of the fly-wheel is to diminish the effect of gravity on the weight in the proportion $\text{M}a^2 : \text{M}a^2 + mk^2$. The measure of the stress on the cord is

$$\text{T} = \frac{\text{MI}g}{\text{M}a^2 + \text{I}} = \frac{\text{M}mk^2g}{\text{M}a^2 + mk^2},$$

and it therefore remains the same throughout the motion. It increases with increase of the radius of gyration of the wheel, but not indefinitely. Its utmost value, as was to be expected, is (Mg) the weight of the appended mass.

135. But the solution of the same problem, by the help of Newton's second interpretation of the third law, is far more simple.

The rate at which the agent (the weight of the falling body) is doing work is, at any instant,

$$\text{M}g\dot{x}.$$

The rate at which energy is being gained by the falling body is $M\dot{x}\ddot{x}$. The rate at which energy is gained by the fly-wheel is $I\omega\dot{\omega}$.

Hence

$$Mg\dot{x} = M\dot{x}\ddot{x} + I\omega\dot{\omega},$$

or by (3), our kinematical condition,

$$Mga^2 = Ma^2\ddot{x} + I\ddot{x}, \quad . \quad . \quad . \quad . \quad (4.)$$

which is the same equation as before.

136. But, instead of reckoning rates of transference of energy, we may still more simply proceed by expressing the conservation of the whole amount of energy in the system (§ 7).

The falling body has *lost* Mgx, and has *gained* $\frac{1}{2}M\dot{x}^2$. The fly-wheel has *gained* $\frac{1}{2}I\omega^2$. Hence

$$Mgx = \frac{1}{2}M\dot{x}^2 + \frac{1}{2}I\omega^2,$$

or by (3)

$$Mgx = \frac{1}{2}M\dot{x}^2 + \frac{1}{2}I\frac{\dot{x}^2}{a^2},$$

which is the fluent, or integral, of (4) when multiplied by $\dot{x}$.

137. If we consider these three solutions of the same problem, we see that, while the stress between the members of the system plays a prominent part in the first, it is altogether unnoticed in the two latter.

This might, at first sight, tend to induce us to ignore stress altogether; and, undoubtedly, we can do so in all cases, *except when we study the condition of the intervening medium, while energy is stored in any part of it; or while energy is being transferred through it from one part of the system to another.* The consideration of this view of the subject is deferred to our chapters on Elasticity. See, especially, § 169.

CHAPTER VII.

GRAVITATION.

138. WITHOUT preface we simply give a statement, compounded from various parts of the *Principia* (especially the *Third Book*), which comprehends all the essentials of Newton's great generalisation.

Every particle of matter in the universe attracts every other particle with a force whose direction is that of the line joining the two, and whose magnitude is directly as the product of their masses, and inversely as the square of their distance from each other.

This statement is made in terms of attraction:—*i.e.* force. Such a form is convenient for our present purpose. But it will be shown later (§ 159) that all we know on the subject can be expressed (and still more simply) in a form which ignores even the very name of force.

It divides itself, for proof, into a number of separate heads; as follows:—

(*a*) The Universality of Gravitation.

(*b*) The direction of the force between two particles.

(*c*) The proportionality of the force to the product of the masses.

(*d*) The law of the inverse square of the distance.

Besides these more immediate assertions the statement also raises the questions—

(*e*) What do we mean by "attraction"?

(*f*) What is the cause of gravitation?

And other matters of great importance naturally present themselves, such as, "What is the mass of the Earth," etc.?

These questions must be kept before us, so that we may give to each of them (so far as our knowledge yet extends, and so far as is consistent with the scope of this work) a sufficient answer. (*f*) is still an open question, for the attempts at answering it have not yet been very successful. (*a*) of course can only be answered either in an *approximate* or in an *indirect* manner, because we cannot (by our most delicate instruments) even prove the existence of gravitation-attraction between two *particles* of matter. Here, however, we tread (as will be seen) on comparatively safe ground.

And the same may be said for (*b*), (*c*), and (*d*), because the reasoning and experiment which sufficiently answer (*a*) will be found here even more complete. (*e*) will be discussed along with (*f*).

139. (*a*) One strong argument for the universality of gravitation is that *the weight of a body is the sum of the weights of its parts.* This is, of course, a matter which can be tested to a very great degree of accuracy by means of the balance. Thus each particle of the body contributes its share to the weight of the whole.

And the weight of a given quantity of matter does not depend upon its form. A mass of gold retains exactly the same weight when it is beaten out into the finest leaf, or dissolved in any quantity, however great, of an acid. Thus terrestrial gravity acts as freely upon the particles when they are surrounded on all sides by the solid mass, as when they are directly exposed by the beating, or solution.

In fact, it is quite easy to see that, were this not the case, were it, in fact, possible to find *a screen through which gravity could not act*, *i.e.* were it possible to interfere with the universality of gravitation, we should also be able to produce *The Perpetual Motion*:—an inexhaustible source of new energy. This we know (§ 7) cannot be.

To show, however, that the above hypothesis would lead to this result, we have only to think of a fly-wheel, one part of which shall be screened from the earth's attraction, the rest unscreened. Every part loses weight as soon as it enters the shadow, as it were, of the screen, and gains it again when it emerges. Thus the wheel, being constantly heavy on one side and weightless on the other, constantly gains energy from nothing.

The wheel would in fact become a tread-mill:—working of itself, instead of by the hard labour of a gang of convicts climbing, without mounting, up one side.

140. (*a*) *continued.* Newton attacked the question by assuming the law of gravitation for the separate particles of a body, and thence finding what should be the law of attraction towards the body as a whole. He thus arrived at two exceedingly beautiful theorems. The first is as follows:—

A spherical shell of uniform gravitating matter exerts no attraction on a particle within it.

[For the proof of this, and of the succeeding proposition, we assume the following results of pure mathematics:—

The area of a transverse section of a cone of small angle is proportional to the square of its distance from the vertex.

The measure of the spherical opening of such a cone is the area it cuts off from the unit sphere whose centre is its vertex; which is the same as the area of the transverse section at unit distance from the vertex.

An oblique section has greater area than the transverse section, at the same distance from the vertex, in proportion to the secant of their inclination to one another.]

Take any point B, within the spherical shell, and let it be the vertex of a double cone of exceedingly small angle. This cuts out two minute areas on the spherical surface, obviously at equal inclinations to the axis of the cone. Hence their areas, and therefore their masses, are as the squares of BP, BQ. But their attractions on B are inversely as the squares of BP, BQ. Thus these attractions balance one another. And the whole shell may thus be divided into pairs of parts, whose attractions exactly balance one another on B. Hence the proposition, which is obviously true of any uniform shell, however thick, if only bounded by concentric spheres. And it is true, if the shell be made up of concentric layers of different densities, provided the density of each layer be uniform.

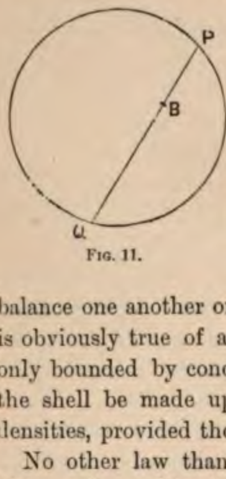

Fig. 11.

No other law than that of the inverse square of the distance is capable of giving this result.

141. The second of Newton's theorems is:—

A spherical shell of uniform gravitating matter attracts an external particle as if its whole mass were condensed at its centre.

Let A be the external particle, C the centre of the shell. Cut off CB, a third proportional to CA, CD; and divide the shell by small double cones whose vertices are at B. Let PBQ be such a cone. Then if ω be its spherical opening, the areas of the sections at P and Q are

$$BP^2\omega \sec CPB,\ BQ^2\omega \sec CPB,$$

and their attractions on unit mass at A are

$$\frac{BP^2.\omega \sec CPB}{AP^2}\rho, \text{ and } \frac{BQ^2.\omega \sec CPB}{AQ^2}\rho,$$

where ρ is the *surface-density*, *i.e.* the mass per unit area.

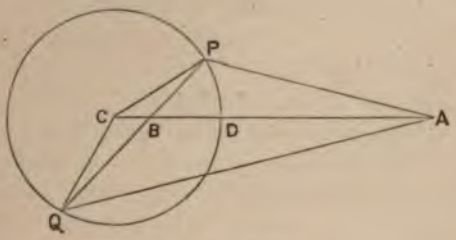

FIG. 12.

But the geometry of the figure shows us at once that $< CPB = < PAD = < QAD$, and $BP : AP :: CP : AC :: BQ : AQ$. Hence the elements at P and Q attract A equally, and the resultant of their attractions is therefore along AC. Its value is

$$\frac{2CP^2.\omega\rho}{AC^2},$$

in which the multiplier of ω is constant; *i.e.* each portion of the shell produces a share, of the whole attraction along AC, proportional to the angular opening it subtends at B.

The sum of all possible values of ω is the area of the surface of the unit hemisphere, *i.e.* 2π. Hence the whole attraction is

$$\frac{4\pi CP^2\rho}{AC^2}.$$

Now $4\pi CP^2$ is the surface-area of the shell, so that the above expression is merely

$$\frac{\text{Mass of shell}}{\text{Square of distance from centre}},$$

and the proposition is proved.

It can at once be extended, as the former was, to a mass made up of concentric shells of different densities, provided each have the same density throughout.

No other law of force, except the law of the *direct* distance, gives this result.

142. Hence a uniform spherical shell, or a mass made up of uniform concentric shells, has a true *Centre of Gravity*, so far as bodies external to it are concerned; for it attracts, and therefore is attracted by, all external bodies, as if it were condensed in its centre.

It is only a limited class of bodies which have a true centre of gravity in the sense just explained. When such a point exists, it always coincides with the centre of inertia, as we see at once by supposing the attracting body to be so distant that its action on different parts of the attracted body is in parallel lines, and proportional simply to the relative masses:—and, for many purposes, it is sufficiently accurate to assume that the centre of inertia of a body may be treated as a centre of gravity.

But we must beware of making too free a use of this hypothesis. If, for instance, the earth had a true centre of gravity, and were rotating about its axis of greatest moment of inertia (through that point), there could be neither Precession nor Nutation.

143. (*a*) *continued.* Armed with these results, Newton was justified in dealing with masses approximately spherical, such as those of the sun and planets, as if each had been a mere particle, condensed at its centre. And here he had the benefit of the altogether extraordinary labours of Kepler; who, by sheer guessing, often of the wildest kind but followed up by persevering calculation, had reduced to a few simple statements the chief *kinematical* results deducible from the observations of Tycho Brahe. These were given in Kepler's work, *De Motibus Stellæ Martis*, Prague, 1609, and are now universally designated

Kepler's Laws.

I. Each planet describes an Ellipse (with comets this may be any Conic Section) of which the Sun occupies one focus.

II. The radius-vector of each planet describes equal areas in equal times.

III. The square of the periodic time (in an elliptic orbit) is proportional to the cube of the major axis.

144. (*b*) Newton showed that, as an immediate consequence of Kepler's Law II. above, the direction of the attraction of the sun for a planet must be that of the line joining their centres.

In fact, double the area described by the radius-vector of a planet in one second is the *moment of its velocity* about the sun's centre. But the moment of the resultant of two velocities is the sum of their separate moments. Hence, as the moment of the planet's velocity remains the same, the moment of each successive increment which it receives must be *nil*, *i.e.* these increments (*i.e.* the accelerations) must be directed towards the sun's centre.

We may prove this part also of the law of gravitation by showing that, were it not true, The Perpetual Motion would be attainable. But the reader may easily make out this proof for himself.

145. (*c*) That the attraction varies directly as the product of the masses will be proved at once if it be shown to be proportional to one of the masses while the other remains constant. For it must be remembered that, by the third law of motion (see § 128), gravitation-attraction is *mutual;* each of the two attracting bodies has as much of a share in producing it as has the other. It is clear, then, that the proof of this part of the law will be obtained at once if we can show that the *weights of bodies are*, in any and every one locality, *proportional to their masses* (§ 34).

We have seen that the measure of a force is the momentum it produces in one second. Submit a number of bodies to the action of their own weights alone, each will acquire in one second a momentum proportional to its weight. But if the weight be proportional to the mass, the momentum must also be proportional to the mass, and thus the speed acquired must be the same for all. That is, if they be under the action, each of its own weight alone, they will fall side by side through any space whatever. Now this is known to be very nearly the case when we let stones or bullets, or even lumps of wood, fall; while it is obviously not so with feathers, paper, or gold leaf. But these exceptions show at once why the trial is not a fair one. The falling bodies are all resisted by the air, some only slightly, others with forces not much less than their whole weights. Hence, to make the experiment as nearly as possible free from such interfering causes, Newton made the fall extremely slow, but in such a way that it could be repeated over and over again under precisely similar circumstances, and therefore its period could be measured very exactly. He used, as the bob of a simple pendulum, a light hollow shell which could be filled successively with different kinds of matter.

In Book II. sec. vi. prop. xxiv. of the *Principia*, he proves that the mass of the bob of a simple pendulum of given length is directly as its weight and as the square of its time of oscillation *in vacuo*. And, in the 7th Corollary to this proposition, we read :—

"Hence appears a method both of comparing bodies one among another, as to the quantity of matter in each, and of comparing the weights of the same body in different places, to know the variation of its gravity. And, by experiments made with the greatest accuracy, I have always found the quantity of matter in bodies to be proportional to their weight."

Thus gravity depends on the *quantity*, but in no way on the *quality*, of the matter in a body; and it is in all cases *attractive*. In these respects it stands in marked contrast to magnetic forces.

146. (*d*) An immediate deduction, from the first two of Kepler's Laws, is that the Hodograph (§ 70) of a planet's orbit is a circle. For (see Fig. 13) the moment of the velocity, V, of P, about the sun, S, is constant (§ 144). And by Kepler's Law I., the orbit ABA′ is an ellipse of which S is one focus. Let fall the perpendicular SQ on the tangent at P, then Q lies on the circle whose diameter is the major axis AA′ of the orbit. Thus V.SQ is constant. But if QS cut the circle again in R, SR.SQ is constant. Thus SR is proportional to V. Hence SR is drawn from a fixed point S, in a direction perpendicular to that of the motion of P, and its length is proportional

to the speed of P. The locus of R, the auxiliary circle, is therefore a curve similar to the hodograph, but turned through a right angle.

The tangent at R, which is the direction of the acceleration of the velocity SR, is therefore perpendicular to SP. [In fact CR is parallel to SP, by a property of the ellipse.] The *magnitude* of the acceleration of P is proportional to the speed of R, *i.e.* proportional to the angular velocity of CR; *i.e.* to the angular velocity of SP. But the moment of P's velocity, about S, which is constant, can also be expressed as the product of SP² into the angular velocity of SP. Hence the angular velocity of SP, and therefore also the acceleration of P, must be inversely proportional to SP². Thus we have the law of change of attraction with distance.

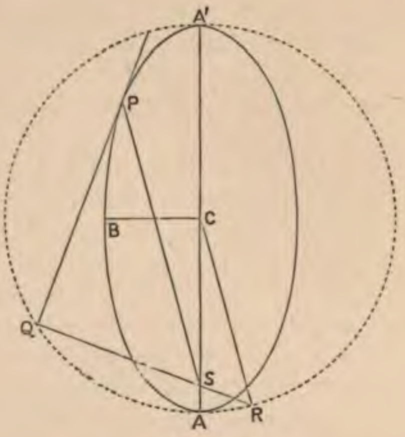

FIG. 13.

147. The detailed investigation is easily given: thus,

if $SP=r$, $<A'SP=\theta$, and if h be twice the area described by SP in unit of time,

$$r^2\dot{\theta}=h.$$

But $$SQ.V=h,$$

while $$SQ.SR=AS.SA'=BC^2,$$

where BC is the semi-axis minor of the ellipse.

Thus $$SR=\frac{BC^2}{h}.V.$$

But, on the same scale, the acceleration of P is measured by the velocity of R, which is $CR.\dot{\theta}$, or $CA.\dot{\theta}$.

Hence the actual acceleration of P is

$$\frac{h}{BC^2}.CA.\dot{\theta}=\frac{h^2.CA^3}{BC^2.CA^2}\frac{1}{r^2}.$$

Now twice the area of the ellipse is $2\pi BC.CA$; and, if T be the periodic time, it must also be $h.T$. Hence

$$\text{Acceleration of } P=\frac{4\pi^2 CA^3}{T^2}.\frac{1}{r^2}.$$

Kepler's Third Law tells us that CA^3/T^2 is the same for all the planets. Hence we conclude that it is the *same* gravitation, diminishing as the square of the distance increases, which acts on each one of the planets.

148. The result of § 146 might at once have been obtained from Kepler's third law. For if we suppose the orbits of the planets to be circles (which they are approximately), that law gives

$$T^2 \propto R^3,$$

where T is the periodic time, R the radius of the circle. But, if V be the planet's speed in its circular orbit, we have the *kinematical* result

$$V^2T^2 \propto R^2.$$

From the two we obtain

$$\frac{V^2}{R} \propto \frac{1}{R^2}.$$

i.e. (see § 121) the accelerations are inversely as the squares of the distances.

But it is better to derive, as Newton did, the law of inverse square from the two first of Kepler's laws; and then the third gives us the further information that every planet behaves exactly as any other would do if substituted for it, *i.e.* that the sun's gravity pays no attention to the *quality* of matter.

149. Having found that, in these general matters at least, the assumed law of gravitation is in agreement with the planetary motions, Newton turned to particulars, and the special one which he took as a test was the moon's revolution about the earth. He says:—

"That the circumterrestrial force likewise decreases in the duplicate proportion of the distances, I infer thus.

. . . . . .

"Let us then assume the mean distance of the moon 60 semi-diameters of the earth, and its periodic time in respect of the fixed stars $27^d\ 7^h\ 43^m$ as astronomers have determined it. And a body revolved in our air, near the surface of the earth supposed at rest, by means of a centripetal force which should be to the same force at the distance of the moon in the reciprocal duplicate proportion of the distances from the centre of the earth, that is, as 3600 : 1, would (secluding the resistance of the air) complete a revolution in $1^h\ 24^m\ 27^s$.

"Suppose the circumference of the earth to be 123,249,600 *Paris* feet, as has been determined by the late mensuration of the French, then the same body, deprived of its circular motion, and falling freely by the impulse of the same centripetal force as before, would, in one second of time, describe 15$\frac{1}{12}$ *Paris* feet.

"This agrees with what we observe in all bodies about the earth. For by the experiments of pendulums, and a computation raised thereon, *Mr Huygens* has demonstrated that bodies falling by all that centripetal force with which (of whatever nature it is) they are impelled near the surface of the earth, do, in one second of time, describe 15$\frac{1}{12}$ *Paris* feet."

The comparatively accurate measurement, of the length of a degree of latitude on the earth, by Picard was undoubtedly the cause which ultimately led to the publication of the *Principia*, of which the fundamental propositions had been obtained nearly twenty years before. For Newton, using the rough estimate of 60 miles to a degree, had found that the moon's deflection by gravity, in one second, from a rectilinear path, was not $\frac{1}{3600}$th of the space through which a stone falls in one second at the surface of the earth, and had in consequence put his investigations aside, until he was led to resume them by hearing the result of Picard's measures.

150. Having thus established the law of gravitation by calculations founded mainly on Kepler's laws, Newton proceeded to show that these laws could not themselves be accurate. For a single spherical planet, revolving about a spherical sun, the first two laws would still be true, but a second planet would at once interfere with this state of matters:—the orbits would no longer be ellipses, and equal areas would no longer be described in equal times. Again, the third law could never be exactly true, even if the planets did not attract one another, unless they contained each the same fraction of the sun's mass. But the consideration of questions like

these belongs to *Physical Astronomy*, with which we have nothing to do here. Suffice it to say that Newton's own magnificently-extended deductions, supplemented as they have been by those of successive generations of illustrious mathematicians, have verified already to a very high degree of nicety the competence of the law of gravitation to account for the excessively complex motions and perturbations observed in the solar system.

151. We have already (§ 118) adverted to the apparent loss of weight by bodies at the equator. This loss, due (in part, at least, for there is another part (§ 165) due to the figure of the earth) to the so-called Centrifugal Force, is, of course, directly proportional to the mass of each body. But experiment with the most delicate balances has shown that bodies of any kind which equilibrate in one latitude equilibrate in all. Hence their weights remain equal when, from that of each, is subtracted an amount proportional to the mass. This can only be if the weights are themselves proportional to the masses. Thus we have an independent experimental proof of the truth of clause (*c*) of Newton's statement.

152. We can scarcely yet be said to have *proof* that gravitation exists, as we know it, in stellar systems. For the data, from which to calculate orbits of double stars, have to be obtained under circumstances which do not admit of more than rude attempts at approximation. We know that there are hundreds of systems in which two or more stars revolve about one another in a way which leaves no doubt that they are *physically connected*. But the observations which have as yet been made have been applied, not to prove that the relative orbits are consistent with Kepler's laws but, to find the approximate dimensions of the orbits, and thence *the amounts of matter in*

the mutually influencing bodies, on the supposition that Kepler's laws hold even in these remote systems.

Thus we cannot, at least for the present, look for proof of the universality of gravitation in *this* direction. But we have ample direct proofs that parts of the earth, and not merely the earth as a whole, exert gravitating force. Some of these will be considered in the immediately succeeding sections.

153. The most direct of these (though not the earliest) is what (though devised by Michell) goes by the name of

The Cavendish Experiment.

In this, by means of the elasticity of a wire or fibre, the attraction between two spheres of manageable size is not only demonstrated, but measured. The following sketch shows a *horizontal* section through the main parts of the arrangement.

Two small balls, A and B, an inch or two in diameter, are connected by a stiff, but very light, horizontal girder or tube, which is suspended at its middle point (E) by a long fine wire. The whole of this part of the apparatus is enclosed in a case, carefully coated with tinfoil or gold-leaf, to prevent (as far as possible) irregular heating and consequent currents of air; perhaps, also, slight electrification. To the girder is attached a small mirror, whose plane is vertical. A little glazed window in the case allows any motion of the mirror to be measured by the consequent deviations of a ray of light reflected by it.

Outside the case are placed two equal, but much more massive, spheres, usually balls of lead a foot or more in diameter, so mounted that they can be made to move (without jerk of any kind) from the positions C_1, D_1, to the positions C_2, D_2, and C_3, D_3, or back again, at will. [In Cornu's recently-constructed apparatus there are four spherical iron vessels, of equal size, placed once for all at

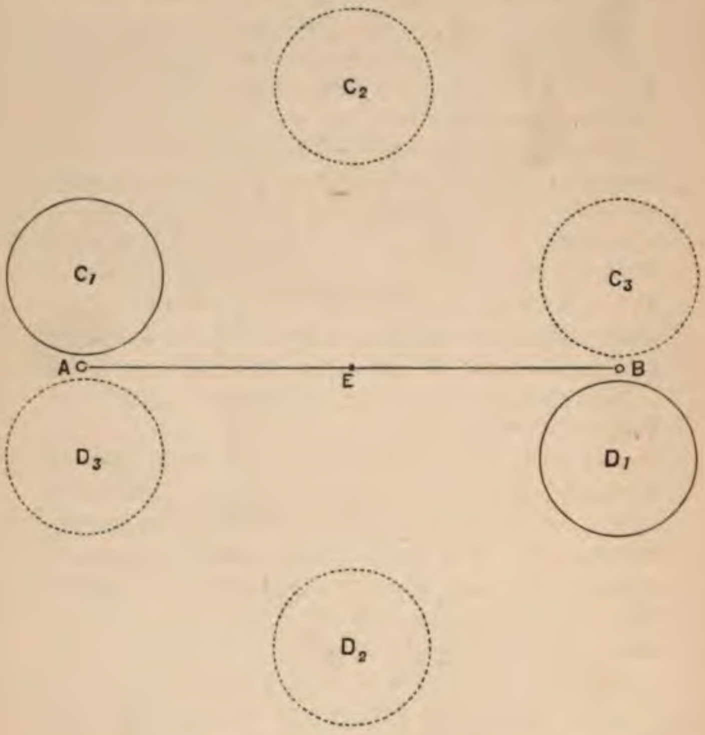

FIG. 14.

C_1, C_3, D_1, D_3, and so connected, two and two, that C_1 or D_3, and simultaneously D_1 or C_3, may be filled with mercury, the other of each pair being left empty. All four can be left empty when required.]

Cavendish, and all who have since made the experiment, found that the apparatus was never at rest. In order to determine the equilibrium position it was necessary, therefore, in all cases to measure the *limits* of successive oscillations, and to compare the mean of two successive deflections to one side, with the intervening deflection to the other side. The time of each oscillation was also carefully measured.

When the large masses were placed at C_2, D_2, in a line perpendicular to the girder (*i.e.* each half-way between its extreme positions), the oscillations were due practically to torsion alone, and the couple required to twist the suspending filament through a given angle could be determined from the period of free oscillation, taken along with the length of the girder and the masses of the two small balls.

When the masses were placed at C_1, D_1, within a couple of inches of the small balls, the range of the oscillation was completely altered. From the observations (made as before) the new position of equilibrium could be calculated. A fresh set of observations was then made with the balls at C_2, D_2, and then they were shifted to C_3, D_3. Thus is determined *the deflection which would have been produced* if the sensitive part of the apparatus could have been reduced to rest.

But from this deflection, and the ascertained coefficient of torsion of the wire, the force acting on each of the small balls can be calculated. This is to be compared with the *weight* of one of the small balls, and then the question is, "What must be the mass of the earth when it attracts a mass at its surface (*i.e.* 4000 miles from its centre) with a force greater in a known ratio than that with which the same mass is attracted by a given spherical mass of lead, whose centre is placed at a given distance?" The law of gravitation at once enables us to write the requisite condition. The mass of the earth, thus found, has only to be divided by its volume (§ 126) to give the mean density.

The quantities compared in such a case, *i.e.* the attractions, may be taken as approximately in proportion to the radius and the mean density of the earth, and of the leaden sphere, respectively. They are, therefore (as the density of lead is double that of the earth), in the ratio $4000 \times 5280 : 2$; or $10^7 : 1$ roughly:—taking 1 foot as the radius of the leaden sphere. Hence, to estimate correctly, to two significant figures only, the earth's mean density, we require to measure a force of the order of the hundred-millionth part of the weight of the small ball. This rough calculation gives some idea of the delicacy of the experiment.

154. The details of the necessary precautions, as well as of the results of various repetitions of this experiment, do not suit a work like this, and must be sought in the original descriptions.[1]

Cavendish's result for the mean density of the earth was 5·48 (the density of water being taken as unit); Reich obtained 5·49; Baily 5·67, since reduced (by the recalculations of Cornu) to 5·55. Cornu's own result is 5·50.

It is very remarkable that Newton, in Book III. of the *Principia*, prop. x., made the following guess:—

"Since the common matter of our earth, on the surface thereof, is about twice as heavy as water, and a little lower, in mines, is found about three, four, or even five times more heavy, it is probable that the quantity of the whole matter of the earth may be five or six times greater than if it consisted all of water."

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[1] Cavendish, *Phil. Trans.*, 1798. Baily, *Mem. Ast. Soc.*, 1843. Reich, *Abhand. d. K. Sächs. Ges.*, 1852. Cornu, *Comptes Rendus*, 1870–78.

Every one of the experimental results, above given, lies almost exactly half-way between the limits thus assigned, and published, more than a century before even the earliest attempt at direct determination was made.

155. Good results have been obtained by a modification of this experiment, which enables the experimenter to employ an ordinary balance; an attracting sphere of considerable mass being applied beneath a sphere attached to one arm of the balance, and already counterpoised (at a different level) by weights in a scale-pan. Thus the uncertainties of torsion are avoided. Of late, however, fibres of quartz have been drawn, which seem to be singularly certain in their working, so that the form of the Cavendish apparatus may perhaps be retained, and its scale very considerably reduced.[1]

156. Other methods, which have been employed for the determination of the mean density of the earth, depend upon the comparison of the attraction exercised by a mountain, or by some other part of the earth, with that of the whole earth, when these act simultaneously, but in different directions, on the same body. The first recorded trial of this method was made by De la Condamine and others, among the Andes. It was first carefully worked out by Maskelyne on a prominent Perthshire mountain, and has consequently been called

The Schehallien Experiment.

By geodetic measures, altogether uninfluenced by gravitation, the actual distance between two stations, one north the other south of the mountain, can be found, and from it can be calculated the difference of their (geographical) latitudes. But the true latitude of each station separately can be determined by the usual astronomical methods, depending on the observed meridian altitude of a star. The difference between the geographical and the true latitude of each station depends upon the attraction of the mountain for the plumb-line, or the trough of mercury, which is used to determine the vertical. The station south of the mountain (in the northern hemisphere) has its latitude made less than the geographical, that to the north made greater by this action. Hence, if everything were symmetrical on the two sides of the mountain, the difference of the astronomically determined true latitudes at the two stations would be *greater* than that of their geographical latitudes by double the deviation produced in the plumb-line by the mountain.

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[1] Boys, *Nature*, xxxix., 65, 1889.

The mountain must now be contoured; then studied by a geologist, so as to enable him to decide on the most probable distribution of matter in it; then the specific gravities of samples of these kinds of matter must be determined. Next a laborious calculation, of the species called *Quadrature*, must be gone through to find its action on the plummet, taking account of the form and density of the mass. Finally, the deflection of the plumb-line is calculated from this result, in terms of the (unknown) mean density of the earth, and compared with the measured deflection.

Maskelyne's[1] observations, developed successively by Hutton[2] and by Playfair,[3] gave as result for the earth's mean density 4·48 and 4·86. The great objection to this

[1] *Phil. Trans.*, 1775. [2] *Ibid.*, 1778. [3] *Ibid.*, 1811.

method is the uncertainty under which we must remain as to the internal structure, not only of the mountain itself but of the whole crust of the earth in its neighbourhood. This cannot be got over completely, so that the result is liable to considerable error.

157. *The Harton Experiment* was made by Airy in the Harton pits. It consists in comparing the intensity of gravity at the earth's surface with that at the bottom of a mine:—the same pendulum being used successively at the two stations; or, still better, two pendulums being made to vibrate simultaneously, one at each station, but now and again interchanged. This method, with the help of modern electrical processes for comparing the behaviour of the pendulums, is probably (so far as exactness of measurement is concerned) a really good one, The intensity of gravity at the bottom of the mine differs from that at the surface on two accounts. Suppose a surface drawn inside the earth, but everywhere at a depth equal to that of the mine; so as to divide the earth into a core, enclosed in a uniformly thick skin, as it were. Gravity at the top of the pit depends on the combined attractions of these parts. At the bottom of the pit the skin (being, at least very approximately, spherical) ceases to attract (by Newton's proposition, § 140), but we have come *nearer* to the core. Hence the observations enable us to compare the attraction of the core with that of the skin. Now we know the volume of the skin, but it has to be assumed (and this is the fatal defect of the method) that the skin is *everywhere* of the mean density determined from examination of the various strata passed through in sinking the pit.

It is not, therefore, surprising that the result of this experiment,[1] viz. 6·56, should differ very materially from the consistent results obtained by the various workers at the Cavendish experiment.

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[1] *Phil. Trans.*, 1856.

158. It was suggested by Robison[1] that the alternate filling and emptying of an estuary or bay, at different states of the tide, might supply an excellent mode of measuring the earth's mean density by means of observations of the consequent twelve-hourly periodic changes of latitude. The contouring required would be very easy, in fact the two chief contours required are given at once by the sea-margins at high and low water; the density of sea-water is practically uniform, and there are places where the whole rise of the tide sometimes amounts to 120 feet or so. But this promising method seems not to have got beyond the stage of suggestion. Still, it is the only one yet proposed, besides the Cavendish method and its mere modifications, which has not some inherent and fatal weakness.

159. (*e*) and (*f*) of § 138 above. That two pieces of matter behave as if they attracted one another according to Newton's law, is certain. But it by no means follows that they do so attract. All that we are entitled to say, from the facts given above, is as follows:—

The part of the energy of a system of two particles of matter, of masses m and m', which depends upon their distance, r, from one another, is less than if they were infinitely far apart by

$$\frac{mm'}{r}$$

and this is not altered by the presence of other particles.

This, taken along with the conservation of energy, enables us fully to investigate the motions of any system of gravitating masses. It represents, in fact, our whole knowledge on the subject. And it is important to observe that the statement is altogether free from even the mention of the word *attraction* or *force*. [See, again, §§ 15, 137.]

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[1] *Elements of Mechanical Philosophy*, 1804, p. 339. See also Forbes, *Proc. R.S.E.*, II. p. 244.

160. We may, however, briefly notice some hypotheses which have been framed as to the mechanism on which gravitation depends. For Newton, in his celebrated *Letters to Bentley*, expressly says:—

"You sometimes speak of gravity as essential and inherent to matter. Pray do not ascribe that notion to me; for the cause of gravity is what I do not pretend to know, and therefore would take more time to consider of it."

"It is inconceivable that inanimate brute matter should, without the mediation of something else which is not material, operate on and affect other matter without mutual contact, as it must do if gravitation in the sense of Epicurus be essential and inherent in it. . . . That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."

161. When we come to deal with molecular forces we shall find that small bodies, such as sticks, straws, air-bubbles, etc., floating on water, are made to aggregate

themselves into groups by molecular tension in the water-surface (§ 288). Hence the idea that stress, in a medium filling all space, might account for the apparent mutual attraction between bodies entirely surrounded by this medium.

Newton, in the Queries at the end of his *Optics*, speaks of a possible explanation to be obtained by assuming that dense bodies rarefy the ether surrounding them, to an amount which is less as the distance is greater.

Clerk-Maxwell says on this point:—[1]

"To account for such a force by means of stress in an intervening medium, on the plan adopted for electric and magnetic forces, . . . we must suppose that there is a pressure in the direction of the lines of force, combined with a tension in all directions at right angles to the lines of force. Such a stress would, no doubt, account for the observed effects of gravitation. We have not, however, been able hitherto to imagine any physical cause for such a state of stress. It is easy to calculate the amount of this stress which would be required to account for the actual effects of gravity at the surface of the earth. It would require a pressure of 37,000 tons' weight on the square inch in a vertical direction, combined with a tension of the same numerical value in all horizontal directions. The state of stress, therefore, which we must suppose to exist in the invisible medium is 3000 times greater than that which the strongest steel could support."

162. Other attempts have been made, with the view of showing that waves, or pulsating motion, in a medium, would have the effect of drawing immersed bodies together. Again, Lord Kelvin has shown that if space be filled with an incompressible fluid, which comes into

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[1] *Ency. Brit.*, ninth ed., Art. "Attraction."

existence in fresh quantities at the surface of every particle of matter, at a rate proportional to its mass, and is swallowed up at an infinite distance, or, if each particle of matter constantly swallows up an amount proportional to its mass, a constant supply being kept up from an infinite distance,—in either case gravitation would be accounted for. This is, however, virtually a suggestion of a dynamical mode of producing the diminution of pressure required in Newton's attempt at explanation.

163. An attempt at explanation, from a totally different point of view, was made by Le Sage in 1818. The following account of it is taken from Clerk-Maxwell's article, "Atom," already referred to:—

"The theory of Le Sage is that the gravitation of bodies towards each other is caused by the impact of streams of atoms flying in all directions through space. These atoms he calls ultramundane corpuscules, because he conceives them to come in all directions from regions far beyond that part of the system of the world which is in any way known to us. He supposes each of them to be so small that a collision with another ultramundane corpuscule is an event of very rare occurrence. It is by striking against the molecules of gross matter that they discharge their function of drawing bodies towards each other. A body placed by itself in free space and exposed to the impacts of these corpuscules would be bandied about by them in all directions, but because, on the whole, it receives as many blows on one side as on another, it cannot thereby acquire any sensible velocity. But if there are two bodies in space, each of them will screen the other from a certain proportion of the corpuscular bombardment, so that a smaller number of corpuscules will strike either body on that side which is next the other body, while the number of corpuscules which strike it in other directions remains the same.

"Each body will therefore be urged towards the other by the effect of the excess of the impacts it receives on the side farthest from the other. If we take account of the impacts of those corpuscules only which come directly from infinite space, and leave out of consideration those which have already struck mundane bodies, it is easy to calculate the result on the two bodies, supposing their dimensions small compared with the distance between them.

"The force of attraction would vary directly as the product of the areas of the sections of the bodies taken normal to the distance and inversely as the square of the distance between them.

"Now, the attraction of gravitation varies as the product of the *masses* of the bodies between which it acts, and inversely as the square of the distance between them. If, then, we can imagine a constitution of bodies such that the effective areas of the bodies are proportional to their masses, we shall make the two laws coincide. Here, then, seems to be a path leading towards an explanation of the law of gravitation, which, if it can be shown to be in other respects consistent with facts, may turn out to be a royal road into the very arcana of science.

"Le Sage himself shows that, in order to make the effective area of a body, in virtue of which it acts as a screen to the streams of ultramundane corpuscules, proportional to the mass of the body, whether the body be large or small, we must admit that the size of the solid atoms of the body is exceedingly small compared with the distances between them, so that a very small proportion of the corpuscules are stopped even by the densest and largest bodies. We may picture to ourselves the streams of corpuscules coming in every direction, like light from a uniformly illuminated sky. We may imagine a material body to consist of a congeries of atoms at considerable distances from each other, and we may represent this by a swarm of insects flying in the air. To an observer at a distance this swarm will be visible as a slight darkening of the sky in a certain quarter. This darkening will represent the action of the material body in stopping the flight of the corpuscules. Now, if the proportion of light stopped by the swarm is very small, two such swarms will stop nearly the same amount of light, whether they are in a line with the eye or not, but if one of them stops an appreciable proportion of light, there will not be so much left to be stopped by the other, and the effect of two swarms in a line with the eye will be less than the sum of the two effects separately.

"Now, we know that the effect of the attraction of the sun and earth on the moon is not appreciably different when the moon is eclipsed than on other occasions when full moon occurs without an eclipse. This shows that the number of the corpuscules which are stopped by bodies of the size and mass of the earth, and even the sun, is very small compared with the number which pass straight through the earth or the sun without striking a single molecule. To the streams of corpuscules the earth and the sun are mere systems of atoms scattered in space, which present far more openings than obstacles to their rectilinear flight.

"Such is the ingenious doctrine of Le Sage, by which he endeavours to explain universal gravitation. Let us try to form some estimate of this continual bombardment of ultramundane corpuscules which is being kept up on all sides of us.

"We have seen that the sun stops but a very small fraction of the corpuscules which enter it. The earth, being a smaller body, stops a still smaller proportion of them. The proportion of those which are stopped by a small body, say a 1 lb. shot, must be smaller still in an enormous degree, because its thickness is exceedingly small compared with that of the earth.

"Now, the weight of the ball, or its tendency towards the earth, is produced, according to this theory, by the excess of the impacts of the corpuscules which come from above over the impacts of those which come from below, and have passed through the earth. Either of these quantities is an exceedingly small fraction of the momentum of the whole number of corpuscules which pass through the ball in a second, and their difference is a small fraction of either, and yet it is equivalent to the weight of a pound. The velocity of the corpuscules must be enormously greater than that of any of the heavenly bodies, otherwise, as may easily be shown, they would act as a resisting medium opposing the motion of the planets. Now, the energy of a moving system is half the product of its momentum into its velocity. Hence the energy of the corpuscules, which by their impacts on the ball during one second urge it towards the earth, must be a number of foot-pounds equal to the number of feet over which a corpuscule travels in a second, that is to say, not less than thousands of millions. But this is only a small fraction of the energy of all the impacts which the atoms of the ball receive from the innumerable streams of corpuscules which fall upon it in all directions.

"Hence the rate at which the energy of the corpuscules is spent in order to maintain the gravitating property of a single pound, is at least millions of millions of foot-pounds per second."

164. One common defect of these attempts is, as Clerk-Maxwell points out, that they all demand some prime-mover, working beyond the limits of the visible universe or inside each atom: creating or annihilating matter, giving additional speed to spent corpuscles, or in some other way supplying the exhaustion suffered in the production of gravitation. Another defect is that they all make gravitation a mere difference-effect as it were; thereby implying the presence of stores of energy absolutely gigantic in comparison with anything hitherto observed or even suspected to exist, in the universe; and therefore demanding the most delicate adjustments, not merely to maintain the conservation of energy which we observe, but to prevent the whole solar and stellar systems from being instantaneously scattered in fragments through space.

In fact, the cause of gravitation remains undiscovered.

165. The ordinary balance, as we have already seen, merely tests equality of masses. To find the *weight* of a body we must measure directly the earth's attraction for it. This can be done, perfectly in principle but only with a rude approximation to accuracy in practice, by means of a *Spring-Balance*, or by some other contrivance which depends on the elastic resilience of a special kind of matter.

By far the most accurate instrument for measuring the intensity of gravity, from which, of course, the weight of any body (whose mass is known) may be immediately calculated, is the pendulum.

A simple pendulum (§ 115) exists, of course, only in theory; but by means of a theorem of abstract dynamics we can calculate the length of the simple pendulum which will vibrate in the same period as does a mass, of any form and dimensions, freely supported in any assigned way on a horizontal axis. This the reader must take for granted.[1] Hence we can reduce observations made with any pendulum to those with the corresponding simple pendulum.

The following expression, whose form is suggested by the theory of the *Figure of the Earth*, and whose constants have been determined and verified by pendulum observations made all over the world, gives approximately the value of g (§ 120) at sea-level in any latitude λ.

$$32{\cdot}088(1+0{\cdot}00513\sin^2\lambda).$$

166. We conclude the chapter with a small table of (approximate) *Specific Gravities*, or what is the same thing (§ 36), *Densities*, and a few remarks suggested by it. None of the numbers for solids can be given with any great accuracy (except perhaps those for natural crystals): for, even if the substance be pure, its density may be altered to a considerable amount by the processes through which it has passed in assuming the state in which it is tested. Such a table as the present must be looked on as affording materials for rough calculations only. When better results are required, special determinations must be made for each substance dealt with.

Hydrogen	0·000089
Steam	0·0006
Nitrogen	0·00125
Air	0·00129
Oxygen	0·00143

(The above are at 1 atmosphere; steam (of course) at 100° C., the others at 0° C.)

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[1] Thomson and Tait's *Elements of Nat. Phil.*, *Appendix*, § *g*.

Cork	0·24
Lithium	0·59
Potassium	0·86
Gutta Percha	0·98
Water	1·00
Magnesium	1·75
Quartz	2·65
Aluminum	2·67
Granite, Marble, Slate	2·7
Glass	2·7 to 4·5
Basalt	2·9
Bromine	3·0
Zinc	7·2
Tin	7·3
Iron	7·8
Nickel	8·7
Copper	8·9
Silver	10·6
Lead	11·3
Mercury	13·6
Gold	19·4
Platinum	21·5
Iridium	22·4

[It is well to note that, as the mass of a cubic foot of water is about 62·5 lbs., these numbers, each multiplied by 62·5, give in pounds very nearly the mass per cubic foot of the corresponding substance.]

The chief additional remark suggested by the table is that, not only are there bodies which, though liquid at ordinary temperatures, are denser than the great majority of solids, but that a comparatively moderate pressure, such as a few hundred atmospheres, would (without producing liquefaction) make the density of air or oxygen greater than that of some solids; so that, for instance, if chemical action could be prevented, we might easily have solid lithium floating upwards in compressed oxygen, as a cork rises in water.

The ratio of the densities of iridium and of hydrogen, as given in the table, is about 250,000:1. But, by means of a Sprengel pump, the density of the hydrogen might easily be reduced to a four-thousandth of its former value. Thus we can place beside one another specimens of matter, one of which has one thousand million-fold the density of the other. Such a comparison may help us to understand the possibility of the existence of the luminiferous medium; which is certainly matter, yet of a density perhaps smaller in comparison with that of attenuated hydrogen, than is the latter in comparison with the density of iridium. In the present work the ether does not come in for treatment. We know it only in so far as it is the vehicle of radiation and electrical energy:—so that it is to works on Light and Electricity the student must be referred.

167. By considering the earth, for a moment, as a liquid mass, it is easy (on hydrostatical principles) to calculate the whole pressure across any plane section of it.[1] This is, of course, the resultant gravitation attraction between the parts separated by the plane of section. Assuming the result of § 154 for the mean density, we find that the average attraction, per square foot, across a diametral plane is about 18×10^8 lbs. weight. The tenacity of sandstone is about 72×10^3 lbs. weight per square foot. Thus gravitation is 25,000 times as effectual in keeping the earth together, as would be its cohesion if it were solid sandstone. Even if the earth were as tenacious as steel, its cohesion across a diametral plane would be only about 1 per cent. of the attraction across it.

Since the cohesion between two halves of a globe is, *ceteris paribus*, as the area of a diametral plane, *i.e.* as the square of the radius, while the gravitation attraction is as the sixth power of the radius directly, and as the square of the radius inversely, a sphere of the earth's mean density and of the tenacity of sandstone would require to be of about 25 miles radius only, in order that cohesion may be as effective as gravity in keeping two hemispheres together. If the tenacity were that of steel, the radius would be about 400 miles.

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[1] Tait, *Proc. R.S.E.*, 1875.

Hence the earth's strength depends almost wholly on gravitation, while that of a stone, less than a mile or so in diameter, depends almost wholly on cohesion, and the more completely the smaller it is.

CHAPTER VIII.

PRELIMINARY TO DEFORMABILITY AND ELASTICITY.

168. A SUBSTANCE is said to be *elastic* when, on being left free, it recovers wholly or partially from a deformation (§ 41).

This definition is sometimes given in another form :—a substance is said to be elastic when it requires the *continued* application of stress to keep it deformed. But this is by no means an equivalent of the former statement; and, besides, it usually introduces complications; for in many substances the force requisite to maintain a distortion becomes less and less with the lapse of time; and the continued application of a given distorting force often produces a constantly increasing distortion. To this, and to another curious property called the *Fatigue of Elasticity*, we will recur, but we will for the present adhere to the first definition given above.

Hence, as an introduction to this part of the subject, we must inquire into the nature and mechanism of the simpler kinds of deformation.

169. The term usually employed for deformation of any kind is *Strain*. The treatment of strains is an entirely geometrical, or (more properly) kinematical, question. But when we inquire how a strain is produced

in a given piece of matter, the question becomes a dynamical one, and we are led to the notion of a system of equilibrating forces, called a *Stress*. (See, again, § 137.) And we figure to ourselves that every stress produces a corresponding strain, which will be of greater or less amount as the specimen of matter operated on is of more or less yielding quality.

It is sometimes convenient to speak of the property of yielding to a particular stress, as when we speak of the *Compressibility* of a substance; sometimes it is more convenient to speak of the property of resistance to a stress, as when we speak of a body's *Rigidity*. But the resistance to a stress is measured by the reciprocal of the amount of yielding (just as the electric resistance of a wire is the reciprocal of its conducting power), so that either of these numerical quantities is immediately deducible from the other.

It will be seen shortly that if P be the measure of any one kind of stress, and p that of the corresponding strain (supposed small), experiment points to a general relation of the form

$$P = Cp,$$

where C is a constant depending on the special substance, and the special form of stress. C is obviously greater, the smaller is the strain for a given stress; and it therefore measures the *resistance* of the substance to the particular kind of stress denoted by P.

As stress is force per unit of surface, while strain has no dimensions, the dimensions of C in the above expression are

$$\left[\frac{M}{LT^2}\right].$$

Hence the numerical value of C changes, in passing from one system of units to another, directly as the unit of length and the square of the unit of time are increased, and inversely as the unit of mass is increased.

170. We shall not require for our elementary treatment of the question more than the simplest portions of the subject of strains, and shall therefore be concerned with *Homogeneous Strain* only.

By this term it is implied that all originally similar, equal, and similarly situated portions of a substance remain after the strain similar, equal, and similarly situated, however their forms and dimensions may be changed. Hence points originally in a straight line, or in a plane, remain in a straight line, or in a plane. Also equal parallel lines remain equal parallel lines. There fore a parallelogram remains a parallelogram, an ellipse remains an ellipse, a parallelepiped remains a parallelepiped, and an ellipsoid remains an ellipsoid.

A most important case is that of a sphere inscribed in a cube. The diameters which pass through the points of contact form a *conjugate* system :—*i.e.* the tangent plane at the extremity of any one of them is parallel to the other two. This parallelism is not affected by the strain; so that when the sphere becomes an ellipsoid, the cube becomes a parallelepiped whose faces are parallel to a set of conjugate planes. Conversely, every set of three conjugate planes of the ellipsoid was originally a set of three mutually perpendicular diametral planes of the sphere. An immediate consequence of this is that *the principal axes of the ellipsoid, which are at right angles to one another, were originally at right angles to one another in the sphere.*

171. Now suppose small, equal, and similarly situated cubes to be traced in the unstrained body. This will be

effected by three imagined series of equidistant parallel planes, those of each series being perpendicular to those of the other two. After the strain the cubes become equal, similar, and similarly situated parallelepipeds. And it is clear that if one of the cubes, and the corresponding parallelepiped, be given, everything else can be determined.

But there is one *special* set, of three series of rectangular planes, with which it is best to commence: viz. those which, as shown in last section, become the principal planes of the ellipsoid which is formed from the sphere inscribed in the cube. This elementary consideration produces a marvellous simplification of our investigation.

172. For we now see that every homogeneous strain may be looked on as having been produced by uniform extensions, or compressions, parallel to three mutually perpendicular lines (the amounts parallel to these being generally different), and a subsequent rotation of the whole as if it were rigid. We shall not require to consider the rotation, for we are concerned only with the deformation which each small part suffers.

Thus, taking account of these permissible simplifications, we need only inquire into the circumstances under which an originally cubical portion of the substance becomes in general brick-shaped, without change of the directions of its edges. The investigation presents no grave difficulties when the strains are of finite magnitude, but we will, for simplicity as well as convenience (§§ 174, 177), consider them as small.

173. We will first consider the particular cases which are of greatest importance.

Nothing need be said of the case where a cube remains a cube, though with altered edges, except that it involves uniform dilatation, or a condensation such as is due to hydrostatic pressure. This is the only strain which does not alter the figure of a body.

But, of the strains which alter the figure of a body, without altering its volume, the most important is that which converts a cube into a brick shape by *lengthening* in a given ratio one set of parallel edges, *shortening* a second set in the same ratio, and leaving the third set unaltered. Here it is obvious that the volume also remains unaltered. Let the ratio of extension be $1+l:1$, that of contraction will be $1-l:1$, on account of the smallness of the fraction l in all the cases which we have to consider. Let the figure represent (in its successive states) one of those faces of the cube, of which all the edges have been altered. The square inscribed in that face is obviously distorted into a rhombus, of which two of the angles are greater, and two less, than right angles, by the same amount, θ suppose.

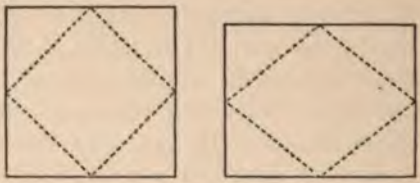
FIG. 15.

Then it is clear that the ratio of the diagonals of the rhombus may be expressed in either of the following forms:—

$$\frac{1-l}{1+l}=\tan\tfrac{1}{2}\left(\frac{\pi}{2}-\theta\right)=\frac{1-\tan\dfrac{\theta}{2}}{1+\tan\dfrac{\theta}{2}}$$

and thus, as θ is very small, so that the arc may be written in place of its tangent,

$$\theta = 2l.$$

This shows the relation between the difference of the angles of the rhombus from right angles, and the fractional alteration of the edges of the original cube.

174. So far for the strain, let us now consider its cause. Every equilibrating system of forces (*i.e.* every stress) can be reduced to simple stresses, each consisting of equal and opposite forces in the same line, that is, *thrusts*, or *tensions*. Thus we have now to inquire what thrusts or tensions will convert a cube of deformable matter into an assigned brick shape: in which, of course, is included the simple case of its remaining a cube, though with altered edges. These must evidently be spread *uniformly* over each of its surfaces, for every one of any number of smaller equal cubes, into which it may be supposed to be divided, suffers precisely the same proportionate deformation.

And as (§ 172) we confine ourselves to very small deformations, any number of them may be superposed, without interfering with one another—*i.e.* they may be successively inflicted in any order, with the same final result. It is mainly for this reason that we restrict ourselves to small strains.

175. The problem is too difficult for an elementary work, unless the portion of matter dealt with be not only homogeneous, but *isotropic*, *i.e.* unless it possess exactly the same properties at all parts and in all directions, so that the effect of a given stress on a unit cube of it is exactly the same however and wherever the cube be cut out of the original material.

Hence we see that, *for cubes which become brick-shaped, without change of direction of the edges*, the thrusts or tensions must each be *perpendicular to the face on which it acts.* And (§ 169) we measure each by its amount per unit area.

[It is most particularly to be remarked that, in all that follows on this subject, it is understood that the body operated on is kept at a *definite temperature*, alike throughout its substance and throughout the whole period of the operation.

The study of the heat developed by sudden applications of stress belongs entirely to *Thermodynamics*, upon which we do not enter in this work. In fact, we here confine ourselves to *Isothermals*, and have nothing to do with *Adiabatics*.]

176. The simplest case of all, and that which alone we require when we deal with fluids, is when the stress is pressure or tension, the same on each face of the cube. Here the cube obviously remains a cube, but its edges are diminished or increased in length. Let unit of edge become $1-f$ (where f is very small) under pressure P per square unit of each face; what is called *hydrostatic pressure*, pressure the same in all directions, and always normal to the surface. Then the volume of unit cube becomes $1-3f$.

The compressibility of an isotropic body is measured by the ratio of the compression per unit volume to the hydrostatic pressure applied.

Hence the compressibility is $3f/\mathrm{P}$, and the Resistance to compression (§ 169), usually called k, is $\mathrm{P}/3f$, so that

$$f=\mathrm{P}/3k.$$

177. When we deal with solids, in which the stress is

not necessarily of the nature of hydrostatic pressure, some further considerations must be attended to.

We now assume, consistently with experiment (as will afterwards be shown), that, if the strain produced by any stress be small, the reversed stress will produce exactly the reversed strain. This is another reason (§ 172) for confining our work to small strains.

Suppose the pairs of opposite faces of a cube be called A, B, and C; the edges joining the corners of each pair a, b, c, respectively. Then a *tension* P, per unit of area, on the A faces will increase a in some definite ratio $1 + p : 1$, and diminish b and c in some common ratio $1 - q : 1$. Now superpose a *pressure* P, per unit area, on the B faces. Since the body is isotropic this will compress b in the ratio $1 - p : 1$, and extend a and c in the ratio $1 + q : 1$. Hence the result of tension P on the A faces and pressure P on the B faces is that a is extended in the ratio $1 + p + q : 1$, b is compressed in the ratio $1 - p - q : 1$, while the length of c is unaltered.

The effect is, therefore, (as in § 173) to change the form of each section of the cube parallel to the C faces, but to leave the area of that section and the volume of the cube unaltered. This strain is called a *Simple Shear*, and the corresponding stress is called *Shearing Stress.*

There is, however, another mode of looking at this matter, to which we must devote a little space. It is usual, in defining Rigidity, to consider the deformation produced in the unit cube by equal *tangential* forces, applied to two pairs of its sides, in directions parallel to the third pair of sides, as indicated in the diagram below. These forces, as shown in the figure, obviously constitute a balancing system, or Stress. But it may be analysed into a much simpler one. For, if we draw either diagonal in the figure, the resultant of the forces applied to either pair of faces on one side of it is easily seen to be P$\sqrt{2}$, in a direction perpendicular to the diagonal. But the length of the diagonal is $\sqrt{2}$. Hence the stress perpendicular to either diagonal plane is P per square unit. And it is clearly a pressure perpendicular to one diagonal plane, and a tension perpendicular to the other. It is therefore the system already studied in § 177, and the effect on the cube above is that studied in § 173.

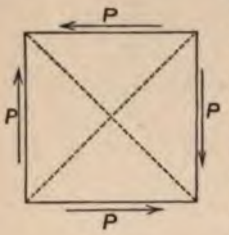

FIG. 16.

178. We now define as follows :—

The rigidity of an isotropic solid (*i.e.* the resistance to change of form under a stress such as that in the above figure) *is directly proportional to the tangential force per unit area, and inversely as the change of one of the angles of the figure.*

Hence, using the common designation, n, and calling θ (as before) the change of each of the angles of the figure, we have

$$\text{Rigidity} = n = \mathrm{P}/\theta,$$

or, by §§ 173, 177,

$$p + q = \frac{\mathrm{P}}{2n} \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

179. But, by § 177, the effect of pressure P, applied simultaneously to all the sides of the cube, would be to reduce the lengths of the edges in the common ratio

$$1 : (1+p)(1-q)^2,$$

or (approximately)

$$1-(p-2q) : 1.$$

Hence by § 176, where it is obvious that f stands for what we now call $p-2q$;

$$p-2q=\frac{\mathrm{P}}{3k} \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

180. From (1) and (2) we have at once

$$p=\mathrm{P}\left(\frac{1}{3n}+\frac{1}{9k}\right),$$

$$q=\mathrm{P}\left(\frac{1}{6n}-\frac{1}{9k}\right).$$

These represent respectively the extension of one set of edges of the unit cube, and the common contraction of the other two, when it is subjected to tension P parallel to the former set.

[These results might have been obtained, perhaps even more simply, by assuming the existence of compressibility with absolute rigidity, then assuming pliability with absolute incompressibility, and superposing the effects. But the logic of this process is more likely to puzzle the beginner.]

181. Hence the extension, per unit of length, of a rod or bar, under longitudinal tension P per square inch of its cross-section, is

$$\mathrm{P}\,\frac{3k+n}{9kn}.$$

The applications of this formula are very numerous and important, as will be seen in § 224, &c., below.

The corresponding diminution, per unit area, of cross-section is

$$\mathrm{P}\,\frac{3k-2n}{9kn}.$$

And thus the increase per unit volume is $\mathrm{P}/3k$, a result which we might have obtained directly in many other ways.

Thus, in pulling out an india-rubber band with a given tension, we *increase* its volume by one-third of the amount by which it would be diminished by hydrostatic pressure of the same value.

Also by pulling out a truly cylindrical and uniform tube, filled to a definite mark with a liquid, we may measure directly the value of k for the matter of the tube.

182. From the foregoing formulæ the result of the application of any (moderate) stress to an isotropic body can be calculated.

As an example, suppose we desire to find what stress will produce extension of an isotropic bar or cylinder unaccompanied by lateral change of any kind.

If we have tensions, P along, and P′ in all directions perpendicular to, the axis of the bar, we have for the longitudinal extension (§ 177)

$$p-\frac{2P'}{P}q\,;$$

and for the extension in any radial direction

$$\frac{P'p}{P}-\frac{P+P'}{P}q.$$

The latter must vanish, by our assumed condition, so that

$$P'=\frac{Pq}{p-q}=P\frac{3k-2n}{3k+4n}\,;$$

which gives the required relation between P′ and P; and thus the extension is

$$P\frac{3}{3k+4n}.$$

183. In the chapters which immediately follow, it

will be seen that to determine the compressibility of a fluid we require (at least in all the ordinary modes of experimenting) to know the distortion produced in the vessel which contains it.

When the same hydrostatic pressure is applied simultaneously to the outside of the vessel and to its contents, the correction for diminution of the interior volume is of course, §§ 176, 212, PV/k:—where P is the pressure per unit surface, V the interior volume, and k the reciprocal of the compressibility of the material of the vessel. This is to be *added to* the apparent compression of the fluid.

But when the pressure on the vessel is mainly internal (as in Andrews' experiments on carbonic acid, § 205), or wholly external (as in glass manometers, § 233), the correction is not so simple. It can, in every case, be determined by means of the equations of § 180; but the investigation even of symmetrical cases is beyond the limits here imposed on us. We therefore merely *state* the results for the forms of vessel most commonly used, viz. tubes and bulbs. For simplicity we assume the tubes to be cylindrical, and the bulbs to be spherical, each being of uniform material and of uniform thickness throughout. The internal and external radii are, in both cases, denoted by a_0 and a_1 respectively; and the cylinders are supposed free to alter in length as well as in cross-section.

Then the diminution per unit of content, by external hydrostatic pressure P, is—

$$\text{In cylinders } P\frac{a_1^2}{a_1^2-a_0^2}\left(\frac{1}{k}+\frac{1}{n}\right),$$

$$\text{In spheres } P\frac{a_1^3}{a_1^3-a_0^3}\left(\frac{1}{k}+\frac{3}{4n}\right).$$

The increase per unit of content, by internal hydrostatic pressure P′, is—

$$\text{In cylinders}\quad P'\frac{a_0^2}{a_1^2-a_0^2}\left(\frac{1}{k}+\frac{a_1^2}{a_0^2}\frac{1}{n}\right),$$

$$\text{In spheres}\quad P'\frac{a_0^3}{a_1^3-a_0^3}\left(\frac{1}{k}+\frac{a_1^3}{a^3{}_0}\frac{3}{4n}\right).$$

When there are simultaneous hydrostatic pressures outside and inside, the corresponding results, calculated from these expressions, are to be simply superposed (§ 174).

Thus, if P and P′ be simultaneous and equal, we have, alike in cylinders and spheres, for the diminution of unit internal content, P/k as above.

When an exceedingly thick vessel (at least a vessel in which a_0^2 is very small in comparison with a_1^2) is exposed to internal pressure only, the effect on unit of its content practically depends on its rigidity alone, and is P'/n for a cylinder, and $3P'/4n$ for a sphere. This is a very striking result.

When such a vessel is exposed to external pressure the result is—

$$\text{For cylinders}\quad P\left(\frac{1}{k}+\frac{1}{n}\right),$$

$$\text{For spheres}\quad P\left(\frac{1}{k}+\frac{3}{4n}\right).$$

This shows the fallacy of the too common notion that, by making the bulb of a thermometer thick enough, we enable it to "*defy pressure*"; as, for instance, when it is to be employed to measure temperatures in a sounding of 3000 or 4000 fathoms.

184. It is very interesting to study the cases of heterogeneous strain presented by the walls of cylinders

and bulbs when the internal and external hydrostatic pressures are different. The following data will show the student the form and volume of the strain-ellipsoid, *i.e.* the ellipsoid into which a very small part of the wall, originally spherical, is distorted. We give the formulæ for a cylinder under external pressure. Let the original position of the centre of the little sphere be at a distance, r (intermediate, of course, between a_0 and a_1), from the axis. Then it is deformed into an ellipsoid, whose axes are—(α) radial, (β) parallel to the axis of the cylinder, (γ) at right angles to these two. If we denote by 1 the original radius of the little sphere, the semi-axes of the ellipsoid are—

$$(\alpha)\quad 1-\mathrm{P}\frac{a_1^2}{a_1^2-a_0^2}\left(\frac{1}{3k}-\frac{a_0^2}{r^2}\frac{1}{2n}\right),$$

$$(\beta)\quad 1-\mathrm{P}\frac{a_1^2}{a_1^2-a_0^2}\left(\frac{1}{3k}+\frac{a_0^2}{r^2}\frac{1}{2n}\right),$$

$$(\gamma)\quad 1-\mathrm{P}\frac{a_1^2}{a_1^2-a_0^2}\frac{1}{3k}.$$

These are, in order of increasing magnitude, (β), (γ), (α). The axes (β) and (γ) are always reduced in length, but the radial axis (α) will be increased in length by the strain provided $r^2 < \frac{3k}{2n}a_0^2$.

In ordinary flint glass this condition becomes, approximately—

$$r^2 < \frac{81}{3}a_0^2.$$

So that the interior layers of a glass tube, exposed to external pressure only, are always extended in the radial direction. This extension is greatest at the interior surface, and vanishes in the layer whose radius is about

$1 \cdot 6 a_0$. If the external radius be greater than this, the outer layers are radially compressed, and the more the farther they lie beyond the limit of no extension.

185. The theory of the propagation of *Waves*, whether of compression or of distortion, in an elastic body, is beyond our limits; but we may make the statement that, if we could set aside the effects of sudden stress in producing changes of temperature, and thus altering the coefficients of compressibility and rigidity (for this question belongs properly to Thermodynamics), the rates of propagation of waves of different kinds depend only upon one or both of the elastic constants (k and n), and upon the density of the body. When the coefficients are measured in terms of the weight of unit bulk of the body, they are called *Moduli*. Hitherto we have measured them in terms of pressure or tension, *i.e.* force per unit area. But, if we measure the force by the length of the column of the substance, of unit section, whose weight it can just support, we obviously take account of the weight of unit bulk. Now the theoretical result (under the conditions above specified) is that the speed of a wave is that which would be acquired by a free body falling, under uniform gravity, through a height equal to half the length of the modulus corresponding to the particular kind of distortion which is propagated. Thus the speed of sound in air or water depends upon the value of k alone; that of a shearing wave, such as light and some forms of earthquake, on n alone. When a wave of extension is sent along a wire, as (for instance) to set a distant railway signal, Young's modulus (§ 224) comes in; and, when we deal with plane sound-waves in a solid, we must take the corresponding modulus as given in § 182.

CHAPTER IX.

COMPRESSIBILITY OF GASES AND VAPOURS.

186. A VERY general proof of compressibility and of elasticity of bulk is afforded at once by the fact that the great majority of bodies are capable of transmitting sound-waves. For the propagation of sound consists essentially in the *handing on* by resilience, from layer to layer of the medium, of a state of compression or dilatation; the (small) disturbance of each particle taking place to and fro in the direction in which the sound is travelling. All ordinary sounds are propagated in air. But the rate of passage of sound has been measured in the water of the Lake of Geneva and elsewhere; and miners are in the habit of signalling to one another by the sounds (of taps with a pick) conveyed through solid rock.

187. Compressibility, elasticity, and inertia of air are all demonstrated by the action of an air-gun. Its reservoir is charged, by means of a pump, with some forty or sixty times the quantity of air which it would contain at the normal pressure and temperature; the moment the valve is thrust down, by the fall of the hammer, a portion of the air is forced out by its elasticity; and this rapid stream, by its inertia, communi-

cates motion to the bullet. The same thing is shown, in a very beautiful form, by allowing the compressed air to escape in a fine jet; for a ball of cork can be suspended in the jet, as a metal shell is suspended in a fountain-jet of water, but in this case without any visible support.

188. In 1662 Robert Boyle published his *Defence of the Doctrine touching the Spring and Weight of the Air.* The following extract, especially, is still of great interest. It occurs in Part II. chap. v.

"We took then a long Glass-Tube, which by a dexterous hand and the help of Lamp was in such a manner crooked at the bottom, that the part turned up was almost parallel to the rest of the Tube, and the Orifice of this shorter leg of the Siphon (if I may so call the whole Instrument) being Hermetically seal'd, the length of it was divided into Inches (each of which was subdivided into eight parts) by a straight list of paper, which containing those Divisions was carefully pasted all along it: then putting in as much Quicksilver as served to fill the Arch or bended part of the Siphon, that the *Mercury* standing in a level might reach in the one leg to the bottom of the divided paper, and just to the same height or Horizontal line in the other; we took care, by frequently inclining the Tube, so that the Air might freely pass from one leg into the other by the sides of the *Mercury*, (we took (I say) care) that the Air at last included in the shorter Cylinder should be of the same laxity with the rest of the Air about it. This done, we began to pour Quicksilver into the longer leg of the Siphon, which by its weight pressing up that in the shorter leg, did by degrees streighten the included Air: and continuing this pouring in of Quicksilver till the Air in the shorter leg was by condensation reduced to take up but half the space it possess'd (I say, *possess'd* not *fill'd*) before; we cast our eyes upon the longer leg of the Glass, on which was likewise pasted a list of Paper carefully divided into Inches and parts, and we observed, not without delight and satisfaction, that the quicksilver in that longer part of the Tube was 29. Inches higher than the other. Now that this Observation does both very well agree with and confirm our *Hypothesis*, will be easily discerned by him that takes notice that we teach, and Monsieur *Paschall* and our *English* friends Experiments prove, that the greater the weight is that leans upon the Air, the more forcible is its endeavour of Dilatation, and consequently its power of resistance, (as other Springs are stronger when bent by greater weights.) For this being considered it wil appear to agree rarely-well with the *Hypothesis*, that as according to it the Air in that degree of density and correspondent measure of resistance to which the weight of the incumbent Atmosphere had brought it, was able to counterbalance and resist the pressure of a Mercurial Cylinder of about 29. Inches, as we are taught by the *Torricellian* Experiment; so here the same Air being brought to a degree of density about twice as great as that it had before, obtains a Spring twice as strong as formerly. As may appear by its being able to sustain or resist a Cylinder of 29. Inches in the longer Tube, together with the weight of the Atmospherical Cylinder, that lean'd upon those 29. Inches of *Mercury*; and, as we just now inferr'd from the *Torricellian* Experiment, was equivalent to them.

"We were hindered from prosecuting the tryal at that time by the casual breaking of the Tube. But because an accurate Experiment of this nature would be of great importance to the Doctrine of the Spring of the Air, and has not yet been made (that I know) by any man; and because also it is more uneasie to be made then one would think, in regard of the difficulty as well of procuring crooked Tubes fit for the purpose, as of making a just estimate of the true place of the Protuberant *Mercury's* surface; I suppose it will not be unwelcome to the Reader, to be informed that after some other tryals, one of which we made in a Tube whose longer leg was perpendicular, and the other, that contained the Air, parallel to the Horizon, we at last procured a Tube of the Figure exprest in the Scheme; which Tube, though of a pretty bigness, was so long, that the Cylinder whereof the shorter leg of it consisted admitted a list of Paper, which had before been divided into 12. Inches and their quarters, and the longer leg admitted another list of Paper of divers foot in length, and divided after the same manner: then Quicksilver being poured in to fill up the bended part of the Glass, that the surface of it in either leg might rest in the same Horizontal line, as we lately taught, there was more and more Quicksilver poured into the longer Tube; and notice being watchfully taken how far the *Mercury* was risen in that longer Tube, when it appeared to have ascended to any of the divisions in the shorter Tube, the several Observations that were thus successively made, and as they were made set down, afforded us the ensuing Table.

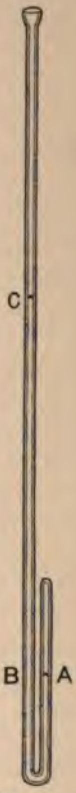

FIG. 17.

"A TABLE OF THE CONDENSATION OF THE AIR.

A.	A.	B.	C.	D.	E.
48	12	00	Added to 29$\frac{1}{8}$ makes	29$\frac{2}{16}$	29$\frac{2}{16}$
46	11$\frac{1}{2}$	01$\frac{7}{16}$		30$\frac{9}{16}$	30$\frac{6}{16}$
44	11	02$\frac{13}{16}$		31$\frac{15}{16}$	31$\frac{12}{16}$
42	10$\frac{1}{2}$	04$\frac{6}{16}$		33$\frac{8}{16}$	33$\frac{1}{7}$
40	10	06$\frac{3}{16}$		35$\frac{5}{16}$	35...
38	9$\frac{1}{2}$	07$\frac{14}{16}$		37...	36$\frac{15}{19}$
36	9	10$\frac{2}{16}$		39$\frac{4}{16}$	38$\frac{7}{8}$
34	8$\frac{1}{2}$	12$\frac{8}{16}$		41$\frac{10}{16}$	41$\frac{2}{17}$
32	8	15$\frac{1}{16}$		44$\frac{1}{16}$	43$\frac{11}{16}$
30	7$\frac{1}{2}$	17$\frac{15}{16}$		47$\frac{1}{16}$	46$\frac{3}{5}$
28	7	21$\frac{3}{16}$		50$\frac{5}{16}$	50...
26	6$\frac{1}{2}$	25$\frac{3}{16}$		54$\frac{5}{16}$	53$\frac{10}{13}$
24	6	29$\frac{11}{16}$		58$\frac{13}{16}$	58$\frac{2}{8}$
23	5$\frac{3}{4}$	32$\frac{3}{16}$		61$\frac{5}{16}$	60$\frac{18}{23}$
22	5$\frac{1}{2}$	34$\frac{15}{16}$		64$\frac{1}{16}$	63$\frac{6}{11}$
21	5$\frac{1}{4}$	37$\frac{15}{16}$		67$\frac{1}{16}$	66$\frac{4}{7}$
20	5	41$\frac{9}{16}$		70$\frac{11}{16}$	70...
19	4$\frac{3}{4}$	45...		74$\frac{2}{16}$	73$\frac{11}{19}$
18	4$\frac{1}{2}$	48$\frac{12}{16}$		77$\frac{14}{16}$	77$\frac{2}{3}$
17	4$\frac{1}{4}$	53$\frac{11}{16}$		82$\frac{12}{16}$	82$\frac{4}{17}$
16	4	58$\frac{2}{16}$		87$\frac{14}{16}$	87$\frac{3}{8}$
15	3$\frac{3}{4}$	63$\frac{15}{16}$		93$\frac{1}{16}$	93$\frac{1}{5}$
14	3$\frac{1}{2}$	71$\frac{5}{16}$		100$\frac{7}{16}$	99$\frac{6}{7}$
13	3$\frac{1}{4}$	78$\frac{11}{16}$		107$\frac{13}{16}$	107$\frac{7}{13}$
12	3	88$\frac{7}{16}$		117$\frac{9}{16}$	116$\frac{4}{5}$

A. A. The number of equal spaces in the shorter leg, that contained the same parcel of Air diversely extended.

B. The height of the Mercurial Cylinder in the longer leg, that compress'd the Air into those dimensions.

C. The height of a Mercurial Cylinder that counterbalanc'd the pressure of the Atmosphere.

D. The Aggregate of the two last Columns, B and C, exhibiting the pressure sustained by the included Air.

E. What that pressure should be according to the *Hypothesis*, that supposes the pressures and expansions to be in reciprocal proportion."

189. The form of apparatus employed by Boyle is still recognised as by far the best for the purpose. With a few necessary modifications, to adapt it to difference of circumstances, it was employed by Amagat[1] in the most important recent experimental determinations of the effects of great pressures on the volume of a gas.

Its action depends on the two hydrostatical principles stated below, the truth of which we are here content to assume.[1]

---

[1] *Annales de Chimie*, 1880.

In a mass of fluid, at rest, the pressure (per square inch) is the same at all points in any horizontal plane.

The change of pressure from one horizontal plane to another is equal to the weight of a column of the fluid, one square inch in section, extending vertically between these planes.

From these it follows that the pressure of the gas operated on, *i.e.* the pressure on the mercury surface at A (Fig. 17) is the same as that at the same level, B, in the other branch of the tube:—and this, again, exceeds the pressure at C (the atmospheric pressure), by the weight of a column of mercury of square inch section and of height BC.

190. In his comments on this experiment Boyle says:—

"For the better understanding of this Experiment it may not be amiss to take notice of the following particulars:—

. . . . .

"3. That we were two to make the observation together, the one to take notice at the bottom how the Quicksilver rose in the shorter cylinder, and the other to pour it in at the top of the longer, it being very hard and troublesome for one man alone to do both accurately.

. . . . .

"6. That when the Air was so compress'd, as to be crouded into less than a quarter of the space it possess'd before, we tryed whether the cold of a Linen Cloth dipp'd in water would then condense it. And it sometimes seemed a little to shrink, but not so manifestly as that we dare build anything upon it. We then tryed likewise whether heat would notwithstanding so forcible a compressure dilate it, and approaching the flame of a Candle to that part where the Air was pent up, the heat had a more sensible operation than the cold had before; so that we scarce doubted but that the expansion of the Air would notwithstanding the weight that opprest it have been made conspicuous, if the fear of unseasonably breaking the Glass had not kept us from increasing the heat.

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[1] See, for instance, Thomson and Tait, *Elements of Natural Philosophy*, §§ 692, 694.

. . . . .

"And there is no cause to doubt, that if we had been here furnished with a greater quantity of Quicksilver and a very strong Tube, we might by a further compression of the included Air have made it counterbalance the pressure of a far taller and heavier Cylinder of *Mercury*. For no man perhaps yet knows how near to an infinite compression the Air may be capable of, if the compressing force be competently increast.

. . . . .

"And to let you see that we did not (a little above) inconsiderately mention the weight of the incumbent Atmospherical Cylinder as a part of the weight resisted by the imprisoned Air, we will here annex, that we took care, when the Mercurial Cylinder in the longer leg of the Pipe was about an hundred Inches high, to cause one to suck at the open Orifice; whereupon (as we expected) the *Mercury* in the Tube did notably ascend. . . . And therefore we shall render this reason of it. That the pressure of the Incumbent Air being in part taken off by its expanding it self into the Sucker's dilated chest; the imprison'd Air was thereby enabled to dilate it self manifestly, and repel the *Mercury* that compress it, till there was an equality of force betwixt the strong Spring of that compress Air on the one part, and the tall Mercurial Cylinder, together with the contiguous dilated Air, on the other part."

It is scarcely necessary to call attention to the truly scientific caution with which Boyle thus gives his conclusions from this notable experiment.

191. BOYLE'S LAW (as it is called in Britain) is now stated in the extended form :—

The volume of a given mass of gas, kept at a given temperature, is inversely as the pressure.[1]

In symbols this is merely

$$pv = C \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

where C is a quantity depending upon the mass of gas, and on its temperature. [This law is only approximately true. In §§ 196–207 below the relation between pressure and volume will be more exactly stated.]

From the definition of density as the quantity of matter per unit of volume, we see at once that Boyle's Law may be stated in the form—

The density of a gas, at constant temperature, is proportional to the pressure.

192. The compressibility follows at once. For a small increase, ϖ, in the pressure, corresponds to a small diminution, ω, in the volume, such that we still have

$$(p+\varpi)(v-\omega) = C = pv.$$

Neglecting the product of the two small quantities we have

$$\varpi v - p\omega = 0.$$

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[1] This Law usually goes by the name of Mariotte in foreign books. See *Appendix IV*,

Here the change, per unit of volume, is ω/v, so that the compressibility (§ 176) is

$$\frac{\omega}{\varpi v}=\frac{1}{p}\,.$$

The resistance to compression is therefore proportional to the pressure. This result was obtained by a graphic process in § 176 above.

193. So closely does air follow Boyle's Law through all ordinary ranges of pressure, that it is constantly used in *Manometers* for the direct measurement of pressure. The manometer is, in its elements, merely a carefully calibrated tube containing dry air, from whose volume (when it is kept at constant temperature) the pressure is at once calculated.

The chief defect of such manometers is that successive equal increments of pressure produce gradually diminishing effects on the volume of the gas; and thus the inevitable errors of observation become more serious, in proportion to the quantity to be measured, as higher pressures are attained. Various ingenious devices, such as tubes of tapering bore, have been devised to remedy this defect. In all such modifications most careful calibration is essential.

194. All gases, at temperatures considerably above what is called their *critical point* (§§ 55, 206), follow Boyle's Law fairly through a somewhat extensive range of pressures. But a gas, at a temperature under its critical point, is really a vapour, and can be reduced (without change of temperature) to the liquid state by the application of sufficient pressure, at least if nuclei be present. The compression of vapours will be treated farther on.

195. So far, we have been dealing with the effects of *increased* pressure. But Boyle carried his inquiry into

the effects of *diminution* of pressure also. His apparatus was of a very simple kind, though still useful, at least for class illustration. The following extract, while highly interesting, sufficiently describes his results and method :—

"A TABLE OF THE RAREFACTION OF THE AIR.

A.	B.	C.	D.	E.
1	$00\frac{0}{0}$	Subtracted from $29\frac{3}{4}$ leaves	$29\frac{3}{4}$	$29\frac{3}{4}$
$1\frac{1}{2}$	$10\frac{5}{8}$		$19\frac{1}{8}$	$19\frac{5}{6}$
2	$15\frac{3}{8}$		$14\frac{3}{8}$	$14\frac{7}{8}$
3	$20\frac{2}{8}$		$9\frac{4}{8}$	$9\frac{15}{12}$
4	$22\frac{5}{8}$		$7\frac{1}{8}$	$7\frac{7}{16}$
5	$24\frac{1}{8}$		$5\frac{5}{8}$	$5\frac{19}{20}$
6	$24\frac{7}{8}$		$4\frac{7}{8}$	$4\frac{23}{24}$
7	$25\frac{4}{8}$		$4\frac{2}{8}$	$4\frac{1}{4}$
8	$26\frac{0}{8}$		$3\frac{6}{8}$	$3\frac{23}{32}$
9	$26\frac{3}{8}$		$3\frac{3}{8}$	$3\frac{11}{36}$
10	$26\frac{6}{8}$		$3\frac{0}{8}$	$2\frac{39}{40}$
12	$27\frac{1}{8}$		$2\frac{5}{8}$	$2\frac{23}{48}$
14	$27\frac{4}{8}$		$2\frac{2}{8}$	$2\frac{1}{8}$
16	$27\frac{6}{8}$		$2\frac{0}{8}$	$1\frac{55}{64}$
18	$27\frac{7}{8}$		$1\frac{7}{8}$	$1\frac{47}{72}$
20	28 +		$1\frac{6}{8}$	$1\frac{9}{80}$
24	$28\frac{2}{8}$		$1\frac{4}{8}$	$1\frac{23}{96}$
28	$28\frac{3}{8}$		$1\frac{3}{8}$	$1\frac{1}{16}$
32	$28\frac{4}{8}$		$1\frac{2}{8}$	$0\frac{119}{128}$

A. The number of equal spaces at the top of the Tube, that contained the same parcel of Air.

B. The height of the Mercurial Cylinder, that together with the spring of the included Air, counterbalanced the pressure of the Atmosphere.

C. The pressure of the Atmosphere.

D. The Complement of B to C, exhibiting the pressure sustained by the included Air.

E. What that pressure should be according to the *Hypothesis*.

"To make the Experiment of the debilitated force of expanded Air the plainer, 'twill not be amiss to note some particulars, especially touching the manner of making the Tryal; which (for the reasons, lately mention'd) we made on a lightsome pair of stairs, and with a Box also lin'd with Paper to receive the *Mercury* that might be spilt. And in regard it would require a vast and in few places procurable quantity of Quicksilver, to employ Vessels of such kind as are ordinary in the Torricellian Experiment, we made use of a Glass-Tube of about six foot long, for that being Hermetically seal'd at one end, serv'd our turn as well as if we could have made the Experiment in a *Tub* or *Pond* of Seventy Inches deep.

"*Secondly*, We also provided a slender Glass-Pipe of about the bigness of a Swan's Quill, and open at both ends; all along which was pasted a narrow list of Paper divided into Inches and half quarters.

. . . . .

"*Fourthly*, There being, as near as we could guess, little more than an Inch of the slender Pipe left above the surface of the restagnant *Mercury*, and consequently unfill'd therewith, the prominent orifice was carefully clos'd with sealing Wax melted; after which the Pipe was let alone for a while, that the Air dilated a little by the heat of the Wax, might upon refrigeration be reduc'd to its wonted density. . . .

. . . . .

"*Sixthly*, The Observations being ended, we presently made the *Torricellian* Experiment with the above mention'd great Tube of six foot long, that we might know the height of the *Mercurial* Cylinder, for that particular day and hour; which height we found to be 29¾ Inches.

"*Seventhly*, Our Observations made after this manner furnish'd us with the preceding Table, in which there would not probably have been found the difference here set down betwixt the force of the Air when expanded to double its former dimensions, and what that force should have been precisely according to the Theory, but that the included Inch of Air receiv'd some little accession during the Tryal; which this newly-mention'd difference making us suspect, we found by replunging the Pipe into the Quicksilver, that the included Air had gain'd about half an eighth, which we guest to have come from some little aerial bubbles in the Quicksilver, contained in the Pipe (so easie is it in such nice Experiments to miss of exactness)."

196. We must now state how far these results of Boyle have been verified by modern experimenters, and in what direction they are found to deviate from the truth. But before we do so we must introduce a definition.

The unit usually adopted for the measurement of pressure is called an *Atmosphere*, roughly 14·7 lbs. weight per square inch.

Its definition is, in this country, the weight of a column of mercury at 0° C., of a square inch in section, and 29·905 inches high; the weighing to be reduced to the value of gravity at the sea-level in the latitude of London. (See § 165.)

The value of an atmosphere, in C.G.S. units, is about 1,014,000 dynes per square centimètre.

197. It is to Regnault that we owe the first really adequate treatment of the subject, but the range of pressures he employed was not very extensive.

Regnault showed that air and nitrogen are, for at least the first twenty atmospheres, *more* compressed than if Boyle's Law were true, but that hydrogen is *less* compressed.

Then Natterer made an extensive but rough series of experiments at very high pressures (sometimes nearly 3000 atmospheres), whose result showed that air and nitrogen, as well as hydrogen, are *less* compressible than Boyle's Law requires, and deviate the more from it the higher the pressure.

198. Andrews,[1] in his classical researches which established the existence of the critical point (§ 55), first

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[1] *Phil. Trans.*, 1869.

gave the means of explaining this very singular fact. We will recur to it when we are dealing with vapours, but we give a few of Andrews' data here. The way in which the compressibility varies with pressure is obvious from the curves in the diagram (§ 205), when interpreted as in § 176. But from Andrews' tables of corresponding volumes of air at 13°·1, and carbonic acid at 35°·5, subjected simultaneously to each of a series of increasing pressures, we extract the numbers in the two first columns :—

Carbonic Acid (Gas) at 35°·5 C.

Recip. of Vol. of Air.	Recip. of Vol. of Carbonic Acid.	pv for Carb. Acid.
81·28	228·0	356
86·60	351·9	246
89·52	373·7	239
92·64	387·9	239
99·57	411·0	242
107·6	430·2	250

Andrews points out that the deviation of air from Boyle's Law is, even at the highest of these pressures, inconsiderable. Taking the reciprocals of the volumes of air, therefore, as measuring pressures with sufficient accuracy, we form the third column of the table. This shows that in carbonic acid, a few degrees above its critical point, the deviation from Boyle's Law is like that in air and nitrogen for the first 90 atmospheres, and, after that, resembles that in hydrogen. Unfortunately the bursting of the tubes prevented Andrews from carrying the pressure beyond 108 atmospheres.

199. The remarkable researches of Amagat already alluded to (§ 189) were carried out in a gallery of a deep coal-pit, where the temperature remained steady for long

periods. The shorter branch of his apparatus, that which contained the gas whose compression was to be measured, terminated in a very strong glass tube of small bore, carefully calibrated. The longer branch was made of steel, and extended to a height of 330 mètres (about 1000 feet) up the shaft of the pit. A small but powerful pump was employed to force mercury into the lower part of the apparatus until it began to run out at one of a set of stopcocks which were inserted at measured intervals along the tall tube. Then a measurement of the volume of the compressed gas was made, the stopcock closed, and that next above it opened in turn for a measurement at a higher pressure.

200. The following short table gives an idea of Amagat's earlier results[1] for air at ordinary temperature :—

Pressure in Atmospheres.	pv.
1·00	1·0000
31·67	·9880
45·92	·9832
59·53	·9815
73·03	·9804
84·21	·9806
94·94	·9814
110·82	·9830
133·51	·9905
176·17	1·0113
233·68	1·0454
282·29	1·0837
329·18	1·1197
400·05	1·1897

[As Amagat's pressure data were obtained direct from a column of mercury, they supply by far the most accurate

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980 ; supplemented from *Comptes Rendus*,

means of finding the unit for pressure gauges. Hence it may be well to note that, at ordinary temperatures, for a pressure of 152·3 atmospheres, or one ton-weight per square inch, dry air almost exactly follows Boyle's Law, *i.e.* it is reduced to 1/152·3 of its volume at one atmosphere. Hence, practically, when dry air is compressed to anything from 1/140 to 1/160 of its bulk under one atmosphere, Boyle's Law may be used to calculate the pressure.]

It is very difficult to assign with exactness the position of the minimum value of pv, as inevitable errors of observation may rise to considerable importance when a quantity varies very slowly; but it may be put down as corresponding to about 78 atmospheres.

201. Amagat's direct measures with the mercury column were made on the volume of nitrogen. But when these had been carefully made, once for all, the nitrogen manometer was used in connection with a similar instrument filled with some other gas. Thus the relation of pv to p was determined with accuracy for hydrogen, oxygen, air (as above), carbonic oxide, carbonic acid, ethylene, etc. In later papers[1] Amagat has extended these results through a considerable range of temperatures. For the numerical data we must refer to the papers themselves; but we reproduce three of the most important of his graphic representations of the earlier results.

The diagram following consists of two parts. The upper part shows the relation of pv to p, through a range of about 80° C., for nitrogen, whose behaviour is typical of that of a large number of gases. The minimum value of pv is distinctly shown at every temperature. The lower diagram exhibits the exceptional case of hydrogen, where all the curves are, practically, straight lines. The pressure unit is a mètre of mercury, *i.e.* 100/76 atmospheres.

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[1] *Annales de Chimie*, xxii. 1881, xxix. 1893.

The diagram on the next page shows the corresponding relations for carbonic acid, at temperatures above its critical point; as well as for liquid carbonic acid at 18°·2 C. In this last case the curve is given only for pressures from 80 to 260 mètres of mercury. This diagram gives very valuable information. Especially it shows the marked influence of change of temperature on the pressure corresponding to the minimum value of pv. Ethylene gives a diagram somewhat resembling this, but the changes in the value of pv are so disproportionately greater that its behaviour could not be satisfactorily exhibited on a scale so restricted as a page of this book.

The reader should be reminded that, had the law of Boyle been accurate, all of these curves would have been simply *horizontal* straight lines.

The more recent researches of Amagat,[1] above referred to, have extended this enquiry to the results of very much higher pressures, such as 3000 atmospheres, under which the density of gaseous oxygen becomes greater than that of water. The exact measurement of these great pressures was effected by means of an exceedingly ingenious instrument, the *Manomètre à pistons libres*, which Amagat constructed for the purpose. In this instrument there are two pistons, of very different sectional area, subjected to the *same total thrust*. Thus the pressure (per square inch) on each is inversely as its section. The pressure on the smaller piston is that of the substance compressed, that on the larger is measured directly by means of a column of mercury. The unit for graduation (which of course depends on the ratio of the effective sections of the

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[1] *Comptes Rendus*, Sept. 1888.

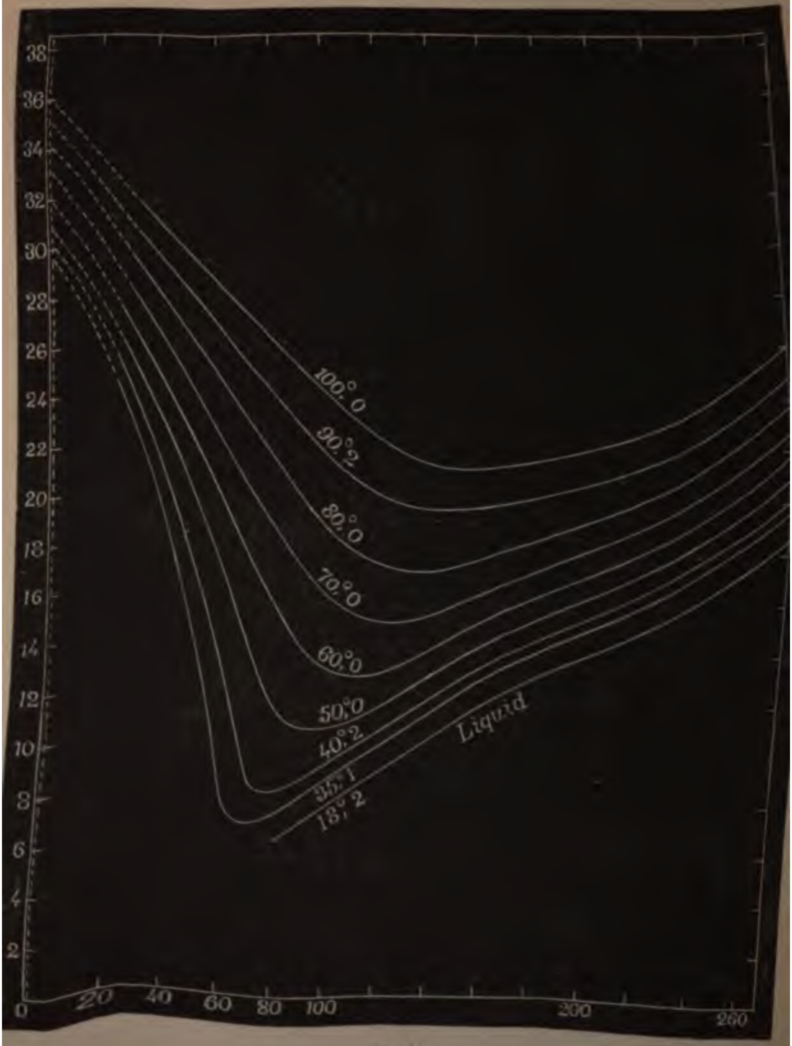
38
36
34
32
30
28
26
24
22
20
18
16
14
12
10
8
6
4
2
0
20
40
60
80
100
200
260
100°.0
90°.2
80°.0
70°.0
60°.0
50°.0
40°.2
35°.1
18°.2
Liquid

Fig. 19.

pistons) was determined, once for all, by comparison with the nitrogen gauge. The special feature of this instrument, on which its precision depends, is that the pistons fit *all but tightly* in their cylinders; a very thin layer of viscous fluid passing with extreme slowness between each piston and its cylinder. Exact adjustment is secured by giving slight rotation to each piston in its bearings. For the larger piston castor-oil is used, for the smaller treacle. But each piston, before being inserted, is most carefully lubricated with neats-foot oil. We have been thus particular in describing the main characteristics of this instrument, because it meets efficiently what has long been felt as an extremely serious want in the physical laboratory.

The pressure which reduced the gas to a given volume was determined by an electrical method which will presently be described (§ 211). In the table below,[1] the volume of each gas at 0° C. and one atmosphere is taken as unit; and the temperature throughout was about 15° C.

True Volumes of various Gases about 15° C. under very great Pressures.

	Volumes.			
Pressure in Atmospheres.	Air. 15°·7.	Nitrogen. 16°·0.	Oxygen. 15°·6.	Hydrogen. 15°·4.
1000	·0020615	·0021340	·0018000	·0017780
1500	17935	18540	15710	14180
2000	16430	16990	14440	12225
2500	15420	15960	13600	11010
3000	14660	15225	12960	10125

Amagat found the compressibility of the glass employed to be about 0·0000023 per atmosphere. As the pressures were equal inside and outside his piezometer, this was the only elastic coefficient required. (§ 183.)

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[1] *Ann. de Chimie*, xxix., 1893.

Thus, referring to § 166, we see that oxygen at 15° has almost reached the density of water at a pressure of 2500 atmospheres. Even at 1000 atmospheres it has about three-fourths of the density of water.

202. There is, unfortunately, a considerable variety of statement as to the relation between pressure and volume in air and other gases, when they are considerably rarefied. This is not to be wondered at, for the experimental difficulties are extremely great.

The experiments of Mendeleeff gave a gradual *descent* of value of pv, in air, from

$$1{\cdot}0000 \text{ at } 0{\cdot}85 \text{ atm.}$$

to

$$0{\cdot}9655 \text{ at } 0{\cdot}019 \text{ atm.}$$

These would tend to show that, at pressures lower than an atmosphere, air behaves as hydrogen does for pressures above an atmosphere.

The experiments of Amagat do not show this result. They rather seem to indicate that pv remains practically *constant* for air, from one atmosphere down to at least $\frac{1}{800}$th of an atmosphere.

203. But the real difficulty in all such experiments arises from the shortness of the column of mercury by which the pressure must be measured. It is not easy to see how this difficulty can be obviated without introducing a chance of graver errors of another kind, due for instance to vapour-pressure or to capillary forces.

We shall find, later, that a fair presumption from Andrews' investigations would be that, in air and the majority of gases, pv should *increase* (of course very slightly) with diminution of pressure from one atmosphere downwards; while (possibly) hydrogen may give values of pv diminishing to a minimum, and then increasing as the pressure is still further reduced.

204. Passing next to the compressibility of vapours, it would appear natural that we should specially consider aqueous vapour, which is constantly present in the atmosphere as *superheated*, sometimes even as *saturated*, steam. And we have for it the splendid collection of experimental results obtained by Regnault. But the critical point of water-substance is considerably higher than the range of temperature in Regnault's work; so that we will deal chiefly with carbonic acid, for which we have Andrews' data both above and below its critical point, and which may be taken as affording a fair example of the chief features of the subject.

205. Without further preface we give Andrews' diagram, which will be easily intelligible after what has been said in § 88. It shows, in fact, how the figure in that section, which is drawn from Boyle's Law, is modified in the case of a true gas, and of a true vapour, each within a few degrees of the critical temperature.

[To save space, a portion of the lower part of the diagram (containing the axis of volumes) is cut away, so that pressures, as shown, begin from about 47 atmospheres. The dotted air-curves are rectangular hyperbolas, as in § 88, but the (unexhibited) axis of volumes is their horizontal asymptote.]

The critical temperature of carbonic acid was given by Andrews as about 30°·9 C., so that the isothermals indicated by full lines in the figure, and marked 13°·1 and 21°·5 respectively, belong to vapour, or liquid, or vapour in presence of liquid, the others to gas. [The true critical point is a little higher (about 31°·3). There was a trace of air, some 1/500th of the whole, in the gas operated on by Andrews.]

Let us study, with Andrews' data, the values of the product *pv* for the isothermal of 13°·1 C. The following table is formed precisely on the same principle as that of § 198 for the isothermal of 35°·5 C.

CARBONIC ACID (VAPOUR AND LIQUID) AT 13°·1 C.

Recip. of Vol. of Air.	Recip. of Vol. of Carbonic Acid.	*pv*. for Carb. Acid.
47·5	76·16	653
48·76	80·43	606
48·89	80·90	600
49·0	105·9	462
49·08	142·0	345
...	...	...
50·15	462·9	108
50·38	471·5	106
54·56	480·4	113
75·61	500·7	151
90·43	510·7	196

206. Near to 49 atmospheres liquefaction commences, the vapour being condensed to $\frac{1}{81}$st of its volume at one atmosphere, and we see that an exceedingly small increase of pressure produces a marked change of volume. Had it been possible to free the carbonic acid perfectly from air, no additional pressure would have been required till the whole was liquid, at about $\frac{1}{463}$d of its original volume. The numbers *pv* diminish, as in the case of air (but much more rapidly), till the liquefaction begins: then they ought to diminish exactly as the volume diminishes (the pressure being constant) till complete liquefaction: after which, of course, they begin to rise rapidly, as it is now a *liquid* which is being compressed.

We need not give the experimental numbers for the isothermal of 21°·5 C.; but the cut shows that the stages of the operation were much the same, only that the pressure had to be raised over 60 atmospheres before liquefaction began, and liquefaction was complete *before* the volume had been reduced so far as at the lower temperature. Thus the range of volume in which the tube was *visibly* occupied partly by liquid, partly by saturated vapour, and therefore (but for the trace of air) necessarily at constant pressure, was shortened at each end. The dotted line in the lower part of the figure, introduced by Clerk-Maxwell, bounds the region in which we can have the liquid in equilibrium with its vapour. This region terminates at the critical isothermal, for above that there can be neither vapour nor liquid.

But the properties of the gas, above the critical point, maintain at any particular volume a certain analogy to those of the vapour or liquid below it. For volumes somewhat above the critical volume (0·0042) the gas has properties analogous to the superheated vapour, *i.e.* pv diminishes with increase of pressure. For volumes somewhat under the critical volume its properties are analogous rather to those of the liquid, and pv increases with increase of pressure. Thus there is in each isothermal of the *gas* a particular pressure, for which pv is a minimum. This feature of the isothermal becomes less marked as the temperature is raised. [This, however, has been already exhibited more fully on Amagat's diagram, p. 177.] We might introduce a continuation, beyond the critical point, of the left-hand portion of the dotted curve, which should pass through the points on each isothermal at which pv is a minimum. This line would divide the wholly gaseous region into two parts; that to its right, in which the gas has properties somewhat resembling those of superheated vapour; to the left, that in which its properties resemble rather those of a liquid.

An ingenious suggestion of J. Thomson substitutes for the horizontal part (liquid in presence of vapour) of Andrews' curves (p. 181) the continuous curve shown (by dashes) on the isothermal of 21°·5 C. The middle portion of this curve (where pressure and volume increase together) is physically unstable, but the other parts can be, to some extent, realised. The subject properly belongs to *Heat*. It is known that liquids may, in certain cases, be raised considerably above their boiling points without boiling; and Aitken (§ 291) has proved that a nucleus of some kind is necessary for the condensation even of supersaturated vapour. The first of these phenomena may account for a portion of the new part of the curve near the liquid region, the second for that near the vapour region. The rest, belonging to an essentially unstable condition, cannot be realised experimentally.

The apparently anomalous behaviour of hydrogen may perhaps be traced to the fact that, at ordinary temperatures and pressures, it is in that region of its gaseous state which has more analogy with the liquid than with the vaporous state. Thus it is possible that if hydrogen be examined at sufficiently low pressure, and temperature not far above its critical point, it also will show a minimum value of pv (§ 203).

207. The reduction of various gaseous bodies to the liquid form was one of the earliest pieces of original work done by Faraday. Some of them he liquefied by cooling alone, many others by pressure alone; and he pointed out that, in all probability, every gas could be liquefied by the combined influences of cooling and pressure, provided these could be carried far enough.

Thilorier prepared large quantities of liquid carbonic acid, and took advantage of the cooling produced by its rapid evaporation, at ordinary pressures, to reduce it to the solid state.

Cagniard de la Tour succeeded in completely evaporating various liquids (including ether, and even water) in closed tubes, which they half-filled while in the liquid state.

It was Andrews' work, however, which first cleared up the subject, and, as an early consequence of it, several of those gases which had resisted all attempts to liquefy them were, at the end of 1877, liquefied:—hydrogen, it is stated, was solidified; but this is extremely doubtful. These important results were obtained by Pictet; and some of them, simultaneously and independently, by Cailletet and v. Wroblewski.

More recently, Dewar has made a careful study of the remarkable properties of air, oxygen, and nitrogen:—obtained in large quantities in the liquid state.

Van der Waals, Clausius, Sarrau, and others, working from various assumptions, have given formulæ which accord somewhat closely with the observed phenomena, and with J. Thomson's suggested modification of the diagram. One of the simplest expressions of the kind (which takes the place of (1) of § 191) is of the form

$$p = \frac{C}{v-\alpha} - \frac{A}{(v-\beta)^2}.$$

Here C is as before, and A, α, β are parameters depending on the properties of the substance as well as on its temperature. The "critical point" is determined by the condition that the three values of v, given by this equation, shall be equal. These formulæ are based upon what is called the *Virial* equation, a result of pure dynamics, given by Clausius in 1870. This represents a relation of equality between the average values of the kinetic energy of a system, and of a certain function of the forces exerted between its parts; which obtains whenever the motion is "stationary". The part of this function which depends on the retaining walls is proportional to *pv*. Hence, when mutual action between the particles is left out of account, we have at once the equation of § 191, C being proportional to the whole kinetic energy of the gas.

But the full treatment of such matters belongs to Thermodynamics, and is not for a work like this. Nor have we anything here to do with the employment of these liquefied gases for the production of exceedingly low temperatures; though, from the experimental point of view, this application promises to be (for the present at least) their most valuable property.

CHAPTER X.

COMPRESSION OF LIQUIDS.

208. A GLIMPSE at the negative results of the early attempts to compress water was given in § 98. The problem is a complicated one, because (at least in the best methods hitherto employed) the quantity really measured is the *difference* of compressibility of the liquid and the containing vessel. Hence it involves the compressibility of solids also:—and this, as we shall find (§ 231) is a very difficult problem indeed. Difficult as it is, it has to be encountered, for the compressibility of glass is by no means small in comparison with that of the greater number of liquids. The first to succeed in proving the compressibility of water was Canton,[1] the value of whose work seems not to have been fully appreciated. His *second* paper, in fact, has dropped entirely out of notice.

Noting the height at which mercury stood in the narrow tube of an apparatus like a large thermometer, immersed in water at 50° F., the end of the tube being drawn out to a fine point and *open*, he heated the bulb till the mercury filled the whole, and then hermetically sealed the tip of the tube. When the mercury was cooled down to 50° F. it was found to have risen in the capillary tube. This e partly to expansion of mercury, released from the pressure of the atmosphere, partly to the compression of the bulb, due to one atmosphere of external pressure. Then he filled the same apparatus with water, performed exactly the same operations, and obtained a notably larger result. This, of course, proves that water (if not also mercury) expands when the pressure of the atmosphere is removed from it.

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[1] *Phil. Trans.*, 1762.

To get rid of the effect of unbalanced external pressure, and thus (as he thought) to measure the *full* amount of expansion, he placed his apparatus (with its end open) in the receiver of an air-pump. He could also place it in a glass vessel, in which the air was compressed to two atmospheres. He observed that, on the relief of pressure, the water rose in the stem, while on increase of pressure it fell. He gives the fractional change of volume per atmosphere, at 50° F. (10° C.), as 1/21740 or 0·000046. He applied no correction for the compressibility of glass, giving the completely fallacious (§ 212) reason that he had obtained exactly the same results from a thick bulb and from a thin one. [This, however, proves the accuracy of his experiments.] His result, considering its date, is wonderfully near the truth.

209. In a second paper,[1] published a couple of years later, Canton made some specially notable additions to our knowledge. For he says, referring to his first paper: "By similar experiments made since, it appears that water has the remarkable property of being more compressible in winter than in summer, which is contrary to what I have observed both in spirits of wine and in oil of olives; these fluids are (as one would expect water to be) more compressible when expanded by heat, and less so when contracted by cold."

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[1] *Phil. Trans.*, 1764, vol. liv. 261.

By repeated observations, at "opposite" seasons of the year, he found that the effect of the "mean weight of the atmosphere" was, in millionths of the whole volume—

	At 34° F.	At 64° F.
Water	49	44
Spirit of Wine	60	71

He also gives a table of compressibilities in millionths of the volume, per atmosphere of 29·5 inches, and of specific gravities; for different liquids, at 50° F.; as follows:—

	Compressibility.	Spec. Gravity.
Spirit of Wine	66	846
Oil of Olives	48	918
Rain Water	46	1000
Sea Water	40	1028
Mercury	3	13595

and he observes that the compressions are not "in the inverse ratio of the densities, as might be supposed."

He calculates from the result for sea water that two miles of such water are reduced in depth by 69 feet 2 inches; the actual compression at that depth being 13 in 1000. This, of course, assumes that the compressibility is the same at all pressures, which, as we shall see immediately, is by no means the case.

210. Perkins, in 1820, made a set of experiments on the apparent compressibility of water in glass, of a somewhat rude kind; but in 1826[1] he gave some valuable determinations, unfortunately defective because of the inadequate measure of the pressure unit. Thus he did not give accurate values of the compression, but he introduced us to a higher problem:—how the compressibility depends upon the amount of pressure. Perkins' results

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[1] "On the Progressive Compression of Water by high Degrees of Force."—*Phil. Trans.*

are all for 50° F. (10° C.), and are given in figures, as well as in a carefully-executed diagram plotted by the *graphic method*. His measurement of pressures depended upon an accurate knowledge of the section of a plunger:—an exceedingly precarious method:—and he estimated an atmosphere at 14 lbs. weight only per square inch. It is not easy to make out his real unit, especially as we know nothing about the glass he used, but it seems to have been about 1·5 times too great; *i.e.* when he speaks of the effect of 1000 atmospheres he was probably applying somewhere about 1500. Hence it is not easy to deduce from his data anything of value as to the *amount* of compression. But the novel point, which he made out clearly, is that (at 10° C.) the compressibility of water decreases, quickly at first, afterwards more slowly, as the pressure is raised. We obtain from Perkins' diagram the following roughly approximate results, in which we have made no attempt to rectify his pressure unit:—

Pressure in Atmospheres.	Compression of Water in Millionths of Orig. Vol.	Average Compressibility per Atmosphere.	True Compressibility per Atmosphere.
150	10,000	66	51
300	17,500	58	48
900	43,400	48	39

and from a further isolated statement we obtain

2,000	83,300	42	...

In this paper Perkins mentions a remarkable experimental result he had obtained:—viz. the *solidification* of acetic acid by pressure. Amagat has recently succeeded in solidifying tetrachloride of carbon by pressure.[1]

211. Örsted's improvement in the experimental method (1822) consisted chiefly in applying pressure, as in Canton's

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[1] *Comptes Rendus*, 1887.

process, in such a way that the effects of pressures up to 40 or 50 atmospheres can be read off at every stage of the experiment.

The liquid operated on fills the bulb and the greater part of the stem of the apparatus (called a *Piezometer*), and is separated by mercury contained in a U tube from the water-contents of a strong glass cylinder, in which the pressure is produced by forcibly screwing in a piston or plug. As in Canton's apparatus, the stem of the piezometer is carefully calibrated and divided into parts corresponding to equal volumes, and the cubic content of the bulb is determined. Hence the ratio of the content of one division of the tube to the whole content of bulb and stem is found.

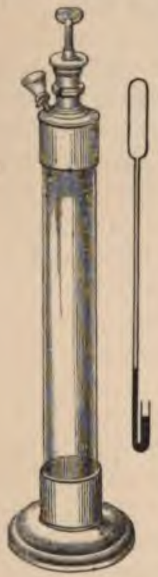

FIG. 21.

When pressure is applied, the mercury is seen to ascend in the stem to an amount nearly in proportion to the pressure. The pressure is roughly calculated (by Boyle's law) from the observed change of volume of air contained in a very uniform tube, closed at the top, and immersed along with the piezometer, in the water of the compression vessel.

The only serious defect of this apparatus, besides the inadequate measurement of pressure, is the limitation of the pressure to what the exterior vessel can resist, some 50 or 60 atmospheres only. When higher pressures are to be applied, iron or steel must be used for the compression vessel; and then the piezometer must be made, in some way, to record the change of volume of its contents.

The most common device is to have (as in a maximum thermometer) a little index resting on the mercury and prevented, by attached hairs, from moving too freely. It contains a small piece of iron, so that it may be adjusted from without by a magnet. This method is liable to the objection that, unless the pressure is relieved very cautiously, the index may be displaced by the current of liquid :—so that the results are sometimes a little too small. Cailletet gilt the inside of the stem, and the eating away of the film of gold showed the height to which the mercury had risen. An exceedingly thin film of silver, deposited by sugar of milk, has also been employed. But all such devices are very troublesome, for the compression vessel has to be opened after every experiment. Hence Tait[1] suggested the sealing of a number of fine platinum wires into the stem of the piezometer, and by an obvious electrical method detecting the instant at which the mercury reaches one of them. Thus, instead of measuring the compression produced by a given pressure, we measure the pressure necessary to produce an assigned compression. This method was employed by Amagat in his later experiments (§§ 201, 217), and he says of it *elle ne laisse réellement presque rien à désirer*.

212. Örsted verified Canton's result that the compressibility of water diminishes with rise of temperature, and suspected that the rate of diminution becomes less as the temperature is further raised; but he did not obtain Perkins' result. In fact he states that at any one temperature the compression is the same, per atmosphere, up to 70 atmospheres.

Örsted, and too many who have followed him, held the opinion that, if the walls of the piezometer were very thin, its internal volume would suffer no perceptible change under equal interior and exterior pressures. That this (like the somewhat similar notion of Canton) is a fallacy, we see at once from the consideration of the effect of hydrostatic pressure on a solid (§ 176). If we suppose the solid to be divided into an infinite number of equal cubes, these would be changed into equal but smaller cubes, in consequence of compression. The strained and the unstrained vessel may therefore be compared to two vaults of brickwork, similar in every respect as to number and position of bricks, but such that the bricks in the one are all less in the same ratio than those in the other. From this point of view it is clear that the interior content of the bulb is diminished just as if it had, itself, been a solid sphere of glass.

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[1] *Proc. R.S.E.*, 1884.

Thus the numbers obtained from the piezometer must all be corrected by *adding* the compression of glass under the same pressure.

Another fallacy much akin to this, and which is still to be found in many books, is the notion that by filling the bulb of the piezometer partly with glass, partly with water, and making a second set of experiments, we shall be able to obtain a *second* relation between the compressibilities of glass and of water; and that, therefore, we shall be able to calculate the value of each by piezometer experiments alone. What we have said above shows that this process comes merely to using a piezometer with a smaller internal capacity; and therefore gives no new information.

If we had a substance of known compressibility, say for instance it were *incompressible*, and were partly to fill the cavity of the piezometer with this, we should be able to get the second relation above spoken of.

In fact the piezometer gives *differences* of compressibility only; so that, for absolute determinations with it, we must have one substance whose compressibility is known by some other method.

When very great pressures are applied, the correction of the apparent compressibility is not quite so simple. If e be the true compressibility of the liquid, ϵ that of the piezometer, the ordinary formula is

$$e=\epsilon+m/p$$

where m is the fractional diminution of volume. It is easy to see, however, that the exact relation is

$$e=\epsilon\,(1-m)+m/p.$$

213. Regnault's[1] apparatus, though managed by a master-hand, was by no means faultless in principle. For pressure was applied alternately to the outside and to the inside of his piezometer, and then simultaneously to both.

There are great objections to the employment of external or internal pressure alone, at least in such delicate inquiries as these. For, unless a number of almost unrealisable conditions are satisfied by the apparatus, the theoretical methods (which must be employed in deducing the results) are not strictly applicable. They are all necessarily founded on some such suppositions as that the bulbs are perfectly cylindrical, or spherical, and that the thickness of the walls and the elastic coefficients of the material are exactly the same throughout. These requirements can, at best, be only approximately fulfilled; and their non-fulfilment may (in consequence of the largeness of the effects on the apparatus, compared with that on its contents) entail errors of the same order as the whole compression to be measured. Jamin has tried to avoid this difficulty by measuring directly the increase of (external) volume, when a bulb is subjected to internal pressure; but, even with this addition to the apparatus, we have still to trust too much to the accuracy of the assumptions on which the theoretical calculations are based.

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[1] *Mem. de l'Acad. des Sciences*, 1847.

Finding that he could not obtain good results with glass vessels, Regnault used spherical bulbs of brass and of copper. With these he obtained, for the compressibility of water, the value

0·000048, per atmosphere

for pressures from one to ten atmospheres. The temperature is, unfortunately, not specially stated.

214. Grassi,[1] working with Regnault's apparatus, made a number of determinations of compressibility of different liquids, all for small ranges of pressure.

He verified Canton's specially interesting result, viz. that water, instead of being (like the other substances, ether, alcohol, chloroform, etc., on which he experimented) *more* compressible at higher temperatures, becomes *less* compressible. Here are a few of his numbers.

Temperature C.	Compressibility per Atmosphere.
0°·0	0·0000503
1°·5	515
4°·0	499
10°·8	480
18°·0	462
25°·0	455
34°·5	453
53°·0	441

These numbers, when exhibited graphically, show irregularities too great to be represented by any simple formula. We now know them to be in many cases very far from the truth, but we give them, *not* at all because they are (even yet) frequently referred to as authoritative, but because some important reductions of physical data have been based upon them; so that it is well to furnish the means of correcting such reductions.

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[1] *Ann. de Chimie*, xxxi., 1851.

Grassi assigns, for sea-water at 17°·5 C., 0·94 of the compressibility of pure water, and gives 0·00000295 per atmosphere as the compressibility of mercury. But he asserts that alcohol, chloroform, and ether have their average compressibility, from one to eight or nine atmospheres, at ordinary temperatures, considerably *greater* than the compressibility for one atmosphere. As this result was shown by Amagat to be erroneous, little confidence can be placed in any of Grassi's determinations.

Amagat[1] gave, among others, the following numbers for ether:—

Temperature C.	Pressure in Atmospheres.	Average Compression per Atmosphere.
13°·7	11	0·000168
13°·7	33	0·000152
100°	11	0·000560
100°	33	0·000474

Thus the diminution of compressibility with increase of pressure is always considerable, and it is more marked the higher the temperature.

215. A very complete series of determinations of the compressibility of water (for a few atmospheres of pressure only), through the whole range of temperature from 0° C. to 100° C., has recently been made by Pagliani and Vincentini.[2] Unfortunately, in their experiments pressure was applied to the inside only of the

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[1] *Ann. de Chimie*, 1877.

[2] *Sulla Compressibilità dei Liquidi*, Torino, 1884.

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piezometer, so that their indicated results have to be diminished by from 40 to 50 per cent. The effects of heat on the elasticity of glass are, however, carefully determined, a matter of absolute necessity when so large a range of temperature is involved. But in these experiments one datum (the compressibility of water at 0° C.) has been assumed from Grassi. The results show that the maximum of compressibility, indicated by Grassi as lying between 0° C. and 4° C., does not exist. The following are a few of the numbers, which show a temperature effect much larger than that obtained by Grassi :—

Temperature C.	Compressibility of water.
0°·0	0·0000503
2°·4	496
15°·9	450
49°·3	403
61°·0	389
66°·2	389
77°·4	398
99°·2	409

Thus, about 63° C. water appears to have its minimum compressibility. The existence of a minimum does seem to be proved, but the remarks above show that its position on the temperature scale is somewhat uncertain.

216. Tait[1] has given the following determinations of the average compressibility of *cistern* water, for pressures up to 450 atmospheres, and temperature from 0° to 15° C. The compressibility of the glass of the piezometer was found by direct experiment (§ 232) to be 0·0000026. The hair-index (§ 211) was employed in the piezometer, so that the results were given as probably somewhat too small.

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[1] *Phys. Chem. Chall. Exp.*, vol. ii. part iv., 1888.

COMPRESSIBILITY OF CISTERN WATER.

Pressure in Atmospheres.	Average Compressibility.
1 to 2	$10^{-7}(520 - 3{\cdot}55t + 0{\cdot}03t^2)$
1 to 153	504 3·60 0·04
1 to 306	490 3·65 0·05
1 to 458	478 3·70 0·06

where t is temperature Centigrade.

The experiments were confined to the three last ranges, so that the data in the first line were obtained by extrapolation. They agree, however, fairly well with two isolated results given by Buchanan,[1] viz. :—

0·0000516 at 2°·5, and 0·0000483 at 12°·5 C.,

and they would have agreed almost precisely with the results of Pagliani and Vincentini (§ 215) had these experimenters taken, as their sole datum from Grassi, the compressibility at 1°·5 instead of that at 0° C.

The temperature of minimum compressibility for 1 atmosphere appears to be about 60° C., and is lowered by increase of pressure.

All the numbers in the above table are *fairly* represented by the approximate formula.

$$\frac{0{\cdot}001863}{36+P}\left(1-\frac{3t}{400}+\frac{t^2}{10{,}000}\right).$$

Here the unit for P is 152·3 atmospheres, or one ton-weight per square inch.

[If we take an atmosphere, instead of a ton-weight per square inch, as our unit ; this gives for the average compressibility of water at 0° C., for the first p atmospheres, the expression

$$\frac{0{\cdot}284}{5483+p}.$$

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[1] *Trans. R.S.E.*, 1880.

The constants in this formula were given as probably too small. Amagat's recent results (§ 217) show that they should each be increased by a little more than 4 per cent. only.]

The corresponding formula for sea-water is

$$\frac{0{\cdot}00179}{38+P}\left(1-\frac{t}{150}+\frac{t^2}{10{,}000}\right).$$

The results have been put in the above form for the sake of comparison with the following expression for the compressibility, at 0° C., of solutions of common salt, viz. :—

$$\frac{0{\cdot}00186}{36+s+P}.$$

In this formula s represents the mass of salt dissolved in 100 of water. It appears, from recent experiments, that a somewhat similar result is given by aqueous solutions of other salts, the addition to the denominator of the fraction being *proportional* (not *equal*) to the mass of salt in 100 of water.[1]

Tait gives the average compressibility of mercury for pressures up to 450 atmospheres as about 0·0000036. This is probably a little too small, as Amagat[2] makes it 0·0000039 for the first 50 atmospheres.

217. The final results of Amagat's splendid researches on the compressibility of liquids at enormous pressures, which exceed in accuracy as well as in extent all previous work, have recently been published.[3] The extremely interesting figure opposite gives some idea of their nature and importance. It represents the isothermals of water

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[1] *Proc. R.S.E.*, June 5, 1893.

[2] *Comptes Rendus*, 1889.

[3] *Ann. de Chimie*, 1893.

and of sulphuric ether, up to pressures of 3000 atmospheres, and for temperatures from 0° to 50° C.

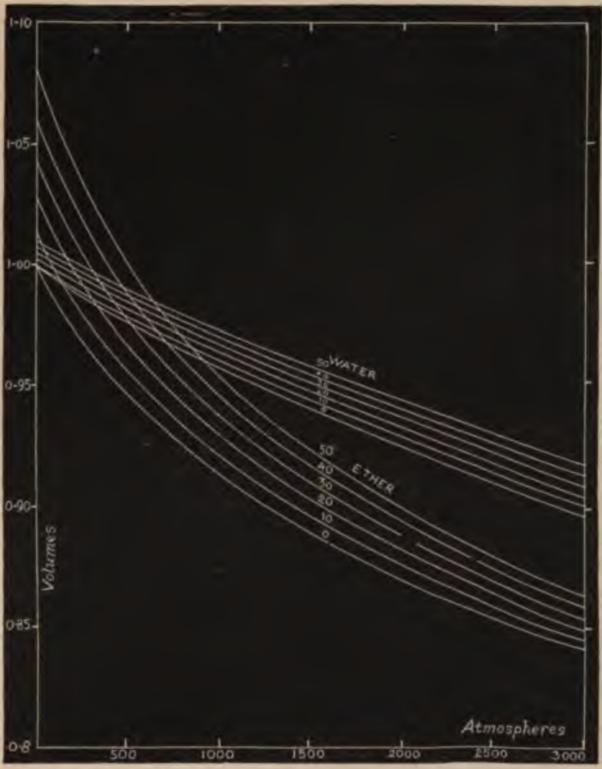

FIG. 22.

From a figure on so small a scale general notions only can be derived. But we see clearly through how small a range of pressures and temperatures the peculiarities connected with the maximum density point of water remain sensible. The quasi-hyperbolic form of the isothermals enables us to make approximate estimates of the utmost compression which these two liquids would suffer under unlimited pressure. More precise information is contained in the following numerical data.

VOLUMES OF WATER AND OF SULPHURIC ETHER UNDER GREAT PRESSURES.

	Water.		Sulphuric Ether.	
Atmospheres.	0° C.	10°·1	0°	20°·2
1	1·00000	1·00015	1·0000	1·0320
500	·97630	·97780	·9465	·9674
1000	·95600	·95850	·9130	·9295
1500	·93890	·94185	·8885	·9020
2000	·92370	·92710	·8684	·8805
2500	·91020	·91370	·8522	·8631
3000	·89830	·90185	·8387	·8485

A convenient and fairly close approximation, deduced from these numbers, gives the following expressions for the average compressibility of water from 1 to p atmospheres :—

$$\text{At } 0^\circ\text{ C.}\quad \frac{0{\cdot}295}{5725+p}$$

$$10^\circ\text{ C.}\quad \frac{0{\cdot}313}{6550+p}$$

This would indicate that water at 0° C. cannot be reduced to less than about 0·7 of its original volume by any pressure, however great.

Amagat's data for ether give the corresponding formulæ.

$$\text{At } 0^\circ\text{ C.}\quad \frac{0{\cdot}2863}{2350+p}$$

$$20^\circ\quad \frac{0{\cdot}302}{2086+p}$$

It is observed that formulæ like these, if they agree exactly with the data for extreme and mid-range of pressures, err slightly in defect for pressures lower than the average, and in excess for those higher (§ 260).

The apparatus which gave the magnificent results tabulated above was, of course, specially adapted to the effects of extreme pressures, and was therefore not qualified to give very precise values for moderate pressures.

218. From the results of Andrews already given (§ 205) we find the following roughly approximate values of the

COMPRESSIBILITY OF LIQUID CARBONIC ACID AT 13°·1 C.

Pressure in Atmospheres.	True Compressibility per Atmosphere.
50	0·0059
60	0·00174
70	0·00096
80	0·00066
90	0·00044

showing very great, but very rapidly decreasing, compressibility. As already explained, Andrews has pointed out that part of this, especially for the lower pressures in the table, is due to the trace of air which, in spite of every precaution, was associated with the carbonic acid.

219. It has long been known that, when the Torricellian experiment is performed, the mercury will sometimes not descend until the tube is sharply tapped, even if the column be 4 or 5 feet in length, instead of being little more than the 30 inches which are supported by the atmospheric pressure. In such a case the portion of the column which stands above the barometric height must be in a state of hydrostatic *tension*. And, as in the case of solids, (§ 177) we conclude that its volume is increased

to the same extent as it would have been diminished by an equal hydrostatic *pressure*.

A very interesting experiment bearing on this subject was made by Berthelot.[1] A strong glass tube, sealed at one end and drawn out very fine at the other, was filled to a definite mark with water. By immersing the whole in warm water the contents were made to expand nearly to the point, which was then hermetically sealed. A very slight additional heating, slowly and cautiously applied, caused the water in time to dissolve the small remaining bubble of air, so that the tube was absolutely full of liquid. When slowly cooled to its original temperature it remained full of water. By the help of the mark (checked if necessary by calculation from the temperature of the warm water) the increase of volume could be estimated, and thence the tension to which the water was exposed. In this way pure water was found capable of bearing some fifty atmospheres of tension, while *eau sucrée* bore nearly one hundred. It is clear that the adhesion of the water to the glass is an indispensable circumstance in this experiment. And as the equilibrium is essentially unstable, throughout the whole contents, it is remarkable that so large an effect can be obtained:—though, of course, it is far below what might (theoretically at least) be supposed possible.

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[1] *Ann. de Chimie*, xxx. 232; 1850.

CHAPTER XI.

COMPRESSIBILITY AND RIGIDITY OF SOLIDS.

220. In the two preceding chapters we had to deal with bodies practically homogeneous (except in the special case of vapour in presence of liquid) and perfectly isotropic; bodies, moreover, which are devoid of elasticity of form, while possessing perfect elasticity of volume. Hence the determination of (apparent) compressibility for any definite substance of these kinds depended for its accuracy solely on the care and skill of the experimenter, and on the adequacy of the process and the apparatus employed.

When we deal with solids the circumstances are very different. It is rarely the case that we meet with a solid which is more than *approximately* homogeneous. Some natural crystals, such as fluor spar, Iceland spar, etc., are probably very nearly homogeneous; so are metals such as gold, silver, lead, etc., when melted and allowed to cool very slowly. To produce homogeneous glass (especially in large discs, for the object-glasses of achromatic telescopes) is one of the most difficult of practical problems. On the other hand, crystalline bodies are essentially non-isotropic; so is every substance, crystalline or not, which shows "cleavage."

And further, very small traces of admixture or impurity often produce large effects on the elastic, as well as on the thermal and electric, qualities of a solid body. Think, for instance, of the differences between various kinds of iron and steel, or of the *purposely* added impurities in the gold and silver used for coinage. Very slight changes, in the manipulation by which wires or rods are drawn from the same material, may make large differences in their final state:—differences by no means entirely to be got rid of by heating and annealing, etc. The whole question of "temper" is still in a purely empirical state. Besides, we must remember that every solid has its limits of elasticity, to which attention must be carefully paid. Thus we can give only general or average statements as to the amount of compressibility or rigidity of any solid, in spite of the labour which Wertheim and many others have bestowed on the subject.

221. In an elementary work we cannot deal, even partially, with the elastic properties of non-isotropic bodies. The necessary mathematical basis of the investigation, though it has been marvellously simplified, is quite beyond any but advanced students. And the experimental study of the problem has been carried out for isolated cases only. Hence we limit ourselves, except in a few special instances, to the consideration of homogeneous, isotropic, solids.

On the other hand, the compression or distortion produced in a solid by any ordinary stress is usually very small. This consideration tends to simplify our work; for, as a rule, small distortions may be regarded as strictly superposable. Thus we may calculate, independently, the effects of each of the simple stresses to which a solid is subjected.

Our warrant for this must of course be obtained experimentally. It was first given by Hooke.

In 1676[1] he published the following as one of "a *decimate* of the *centesme* of the Inventions, etc."—

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[1] *A Description of Helioscopes, &c., made by Robert Hooke*, Postscript, p. 31.

"3. *The true Theory of* Elasticity *or* Springiness, *and a particular Explication thereof in several Subjects in which it is to be found: And the way of computing the velocity of Bodies moved by them.* ceiiinosssttuu."

The key to this anagram was given by Hooke himself in 1678,[1] in the words:—

"About two years since I printed this Theory in an Anagram at the end of my Book of the descriptions of Helioscopes, viz. *ceiiinosssttuu, id est, Ut tensio sic vis;* That is, The Power of any Spring is in the same proportion with the tension thereof: That is, if one power stretch or bend it one space, two will bend it two, and three will bend it three, and so forward. Now as the Theory is very short, so the way of trying it is very easie."

He then shows how to prove the law in various ways:—with a spiral spring drawn out; a watch spring made to coil or uncoil; a long wire suspended vertically and stretched; and a wooden beam fixed (at one end) in a horizontal position, and loaded.

The above extracts sufficiently show in what sense Hooke intended the words *Tensio* and *Vis* to be understood:—and his law is now usually stated in the (somewhat amplified) form, called HOOKE'S LAW,

Distortion is proportional to the distorting Force,

or, still more definitely,

Strain is proportional to Stress.

In the latter form we have made anticipatory use of it in Chap. VIII. and elsewhere.

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[1] *Lectures de Potentia Restitutiva, or of Spring,* p. [1]. This is a very curious pamphlet, containing some remarkably close anticipations of modern theories, especially *Synchronism* and its results, and the *Kinetic Theory of Gases.* The first is foreign to our present subject, the second will be considered later (§ 322).

222. A very general proof of the accuracy of this law is easily to be obtained in the case of bodies which can be made to produce a musical sound :—a tuning-fork, for instance. For, if the *pitch* of the note (*i.e.* the number of vibrations per second) do not alter as the sound grows fainter, the vibrations must be isochronous, and the elastic resilience therefore proportional to the distortion. (See § 72.)

223. The ordinary experimental illustrations of Hooke's Law are given, very much as he originally gave them, by :—

1. A rod or wire, fixed vertically and stretched by appended weights; or a rod or column compressed by weights laid on its upper end.

2. A wire stretched horizontally and extended by weights suspended at its middle point.

3. A bar or plank fixed horizontally at one end and loaded with weights at the other.

4. A plank with its ends resting on trestles and loaded at the middle.

5. A spiral spring, forming a helix of *small* step, compressed or extended by weights.

6. A wire or rod, fixed at one end and twisted at the other.

The mere mention of these methods is sufficient, without further illustration, to suggest the means by which the requisite measurements can be carried out. They will be considered in detail, but not in the above order.

In all these cases experiment shows that (within certain limits, which will be afterwards discussed) the distortion is proportional to the distorting force.

1 and 2 are mere varieties of one experiment. The same may be said of 3 and 4, which are examples of a

somewhat more complex form. And 5 and 6, though at first sight very unlike, are practically one problem. Besides, they are of a simpler character than either of the other pairs, for they involve the coefficient of rigidity alone; the others involve both coefficients. But 1 and 2, on the other hand, are simpler than the rest, on a different account, viz. that they involve *homogeneous* strain.

224. *Young's Modulus*, as it is called, is determined from the stretching of a rod or wire by appended weights. As defined by Young, its measure is the ratio, of the simple stress required to produce a small shortening or elongation of a rod of unit section, to the fractional change of length produced. Its value is expressed, as we see by § 181, in terms of the rigidity and the resistance to compression, by the formula

$$\frac{9kn}{3k+n}.$$

For bodies like india-rubber, in which k is large in comparison with n, its value is nearly $3n$. Hence the pulling out of an india-rubber band is almost entirely due to change of form (see again, § 181) and therefore the area of a cross section is diminished in nearly the same proportion as that in which the band is lengthened.

A piece of good cork suggests, though it does not realise, the conception of a solid in which n shall be very large in comparison with k; and for such a body Young's modulus would be nearly $9k$. Since n is very large, there is little change of form; so that traction or pressure, in any direction, would expand or contract a body of this kind nearly equally in all directions. In cork the effect is confined mainly to the dimension operated on.

From such considerations we see that Young's modulus, though comparatively easy of measurement, is not the simple quantity which it at first appears to be; and that, in fact, it may have the same numerical value in each of two bodies which differ widely from one another, alike in rigidity and in compressibility.

225. The following table gives approximate values (§ 166) of Young's modulus for some common materials; the unit being 10^7 grammes' weight per square centimètre :—

	Young's modulus, $\frac{9kn}{3k+n}$.	Tenacity.
Gold	86	0·27
Silver	76	0·3
Copper (hard)	125	0·4
Copper (annealed)	110	0·3
Iron	180	0·6
Steel	240	0·8
Oak	10	0·1
Teak	17	0·1
Fir	12	0·07
Glass	40 to 60	0·06

To convert these numbers (as they stand in the table) into the common reckoning of pounds' weight per square inch, it suffices to multiply them by about 142,000 instead of by 10^7. To convert to C. G. S. units, *i.e.* dynes per square centimètre, multiply by $9{\cdot}81 \times 10^9$.

226. A second column (in terms of the same units) has been added to the above table, to give an indication of the *Tenacity* of each of the materials specified. This means the utmost longitudinal stress which (when cautiously applied) a rod or wire can endure without rupture. It has no direct connection with Young's modulus, nor with either of the coefficients of elasticity, for a substance has usually to be strained far beyond its limits of elasticity before rupture takes place, and the

dimensions of the cross section are also much reduced. The uncertainty of the amount of this quantity, even in different specimens taken from the same piece of matter, leads to our giving it usually to one significant figure only.

227. Young's treatment of the subject of elasticity is one of the few really imperfect portions of his great work.[1] He gives the values of his modulus for water, mercury, air, etc.! It is not easy to understand what he really meant by speaking of "*the*" modulus of elasticity: unless, as Lord Rayleigh suggests, he meant that which (whatever be, in each case, its real nature) is involved in ordinary sound waves, whether in air or along wires. Young's modulus is, no doubt, a quantity of great value in practical engineering:—in many cases the only elastic datum required. Yet he speaks of rigidity, etc., in a way which is scarcely compatible with the idea of one modulus only. But the subject was in a state of great confusion till long after his time, mainly in consequence of an unwarranted conclusion (deduced by Navier and Poisson from a species of molecular theory) that there is a necessary numerical ratio between rigidity and resistance to compression. In fact, what was called Poisson's ratio, that of the lateral shrinking, to the longitudinal extension, of a bar or rod under tension, was supposed to be necessarily equal to 1/4. This gives (§ 180) $p=4q$, or $3k=5n$.

The erroneousness of this conclusion was first pointed out by Sir G. G. Stokes,[2] and his paper has put the whole subject in a new and clear light. We have already given, in § 224 above, some of his illustrations, which show

[1] *Lectures on Natural Philosophy*, 1807.

[2] *Camb. Phil. Trans.*, 1845.

that there is no necessary ratio, or even relation, between n and k.

De St Venant[1] has given *complete* solutions of a number of interesting cases, such as the torsion of prisms of different forms of cross-section, many of which are very valuable in practical applications. Lord Kelvin,[2] besides giving the theory with extreme generality, has also specially developed the application of *Thermodynamics*[3] to the subject.

In spite of Stokes' exposure of the inaccuracy of the so-called *Uni-constant Theory*, it has still determined partisans. These may profitably consult the following data, given by Amagat;[4] though we quote them for their intrinsic value, not for the purpose of further "slaying the slain."

Elastic Constants (Mean Values) at 12° C.

	Poisson's Ratio.	Compressibility per Atmosphere.	Young's Modulus.
Glass	0·245	0·00000220	6,775
Steel	0·268	68	20,395
Copper	0·327	86	12,145
Brass	0·327	95	10,851
Lead	0·428	276	1,556

The unit for Young's modulus, which was determined directly, is a kilogramme weight per square millimetre, so that the numbers in the last column must be divided by 100, to reduce them to the unit employed in the table of § 225.

The numbers in the first two columns are the means of closely accordant results derived, one set from the change of contents of a cylinder under longitudinal traction (§ 181), the other from the similar change under external pressure alone (§ 183). Along with each of these data the value of Young's modulus, as given in the last column, was employed.

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[1] *Mém. des Savans Étrangers*, 1855. See also Thomson and Tait's *Nat. Phil.*, §§ 699, etc.

[2] *Phil. Trans.*, 1854.

[3] *Quarterly Math. Journal*, 1855.

[4] *Comptes Rendus*, 1889.

228. We will now consider the pure *Torsion* of a cylindrical rod or wire, as employed, for instance, in the Cavendish experiment (§ 153).

This is a very simple problem if the cylinder be truly circular, and of perfectly homogeneous isotropic material. For it is clear from what follows that equal and opposite twisting couples, applied at its ends, will simply make successive transverse slices, of equal thickness, rotate about the axis each by the same amount less than the one before it.

The length of the cylinder cannot increase under torsion, for a reversal of the couples (which is practically the same arrangement) would shorten it (§ 177), and *vice versâ*. Neither can its radius change, for exactly the same reason. Nor can a transverse section become curved at any part. Thus the volume remains unchanged, and therefore the coefficient of rigidity alone is involved.

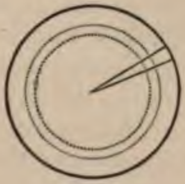

FIG. 23.

Consider a thin annular portion of the cylinder bounded by transverse sections at a very small distance, t, from one another, and by concentric cylinders of radii r, and $r+t$. We may subdivide this into cubes, of side t, by planes through the axis, making angles t/r with one another.

Let φ be the twist per unit length of the cylinder, $t\varphi$ is the angle by which one of the parallel sections has rotated relatively to the other, and $r.t\varphi/t$, or

$r\phi$, is the change of angle in each of the little cubes. Hence, if P be the tangential force per unit of area (§ 178)

$$P = nr\phi.$$

The moment, about the axis, of the tangential force on the cube is therefore

$$Pt^2 . r = nr^2t^2\phi.$$

[Note here, for an ulterior purpose (§ 235), that r^2t^2 is the moment of inertia (§ 132) of the *area* of the face of the cube about the axis.]

But the number of cubes is $2\pi r/t$, so that the whole moment is $2\pi nr^3t\phi$.

This is the couple required to twist a circular cylinder of radius r, and very small thickness t, through the angle ϕ per unit of length.

To find the result for a solid cylinder of radius R, we must put dr for t, and integrate. The result is

$$\frac{\pi}{2}nR^4\phi.$$

Hence the twist produced, per unit of length in a cylinder, is directly as the twisting couple; inversely as the rigidity and as the fourth power of the radius.

229. This suggests an obvious and direct experimental process for determining the rigidity of homogeneous isotropic substances. There are two difficulties, of a formidable character, in the way of its application: first, the obtaining a homogeneous isotropic material, and secondly, the making it into a circular cylinder. It is clear that very small irregularities of form, or errors in the estimate of the radius, may give rise to large errors in the calculation of the rigidity, since the *fourth* power of the radius is directly involved in the calculations. And it is probable that the mode of manufacture of the

cylinder (especially if it be drawn, § 49) may render its otherwise isotropic material markedly non-isotropic. Hence the following numbers are given as mere approximations. The unit is, again, 10^7 grammes' weight per square centimetre (§ 225).

APPROXIMATE RIGIDITY (n).

Glass	15 to 25
Brass	35
Iron (wrought)	79
Iron (cast)	55
Steel	85
Copper	45 to 50

These values are for ordinary temperatures. As the temperature is raised, the rigidity is found steadily to diminish.

230. When a spiral spring is drawn out, it is pretty clear to every one that there is unbending, for the curvature becomes less as the helix is lengthened. And the following simple experiment shows that this flexure is accompanied by torsion. Coil up a strip of sheet

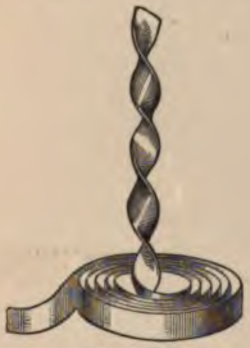
FIG. 24.

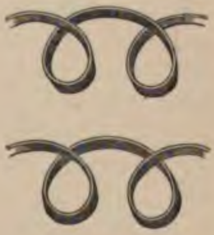
FIG. 25.

india-rubber, as in the cut, and pull out the inner end. It assumes the form sketched. The portion pulled out straight is twisted merely; the coiled part is merely bent; the intermediate portion is partly bent and partly twisted. Every coil pulled out gives one complete turn of twist. If we make *kinks* on the strip, as in fig. 25, then, on pulling out the first, we find *two* complete turns of twist, but on pulling out the second there is *no* twist, one of the kinks giving a right-handed, the other a left-handed, complete turn of twist.[1]

When the spring is very flat, *i.e.* has a very small step, the principal effect of a moderate extension is mere torsion; and the investigation is of a character precisely the same as that in the preceding section. The somewhat more complex combination, of torsion and flexure simultaneously, will be adverted to later. (§ 237.)

231. Theoretically speaking, we can of course deduce the resistance to compression from the (known) values of the rigidity and of Young's modulus; and it is in this way that most of the data connected with the subject have been obtained. But especially in cases where Young's modulus is not very far from threefold the rigidity (as, for instance, in india-rubber), the inevitable errors in the determination of these might lead to enormously greater errors in the calculated value of k.

The method which was incidentally employed by Regnault, in his measurements of the compressibility of liquids, consisted in applying pressure externally, internally, and externally and internally, to a species of piezometer containing water. The results of § 183 show that (supposing it cylindrical, and unit pressure applied) its internal volume must have been altered, in these three cases respectively, by the following fractions of its whole amount:—

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[1] Knots. *Trans. R.S.E.*, 1877.

$$
\begin{gathered}
-\frac{a_1^2}{a_1^2-a_0^2}\left(\frac{1}{k}+\frac{1}{n}\right) \\
\frac{1}{a_1^2-a_0^2}\left(\frac{a_0^2}{k}+\frac{a_1^2}{n}\right) \\
-\frac{1}{k}
\end{gathered}
$$

The algebraic sum of the two first is of course equal to the third. But the quantities measured in the two latter cases were both less than those stated above by the fractional change of volume of the water. The relation, therefore, still holds, and furnishes a test of the accuracy of the experiments. But it reduces the number of independent equations to two, from which there are three coefficients of elasticity to be determined. Hence Regnault also had to fall back on the employment of Young's modulus.

An interesting illustration of the above statements is furnished by an experiment of Forbes. He replaced, by an india-rubber bottle, the bulb of a piezometer. In such an instrument the apparent compressibility of water was found to be barely sensible.

232. Probably the best, certainly the most direct, method is that adopted by Buchanan,[1] in which the length of a rod is very carefully measured while it is under hydrostatic pressure, and also while free. The linear contraction so determined is numerically (if the material be homogeneous and isotropic) one-third of the compression (§ 176). Unfortunately Buchanan's published measures are confined to one particular kind of glass. The special merit of his method is that, provided

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[1] *Trans. R.S.E.*, 1880.

the rod be of isotropic material, the regularity of its cross section is of no consequence.

Thus we can give for this property also only a few roughly approximate numbers. They are given in the same units as the preceding.

APPROXIMATE RESISTANCE TO COMPRESSION (k).

Glass	20 to 40
Copper	160
Iron (wrought)	150
Steel	185 to 200

It is greatly to be desired that more, and more accurate, data should be obtained in this matter:—though, as is evident from § 219, the problem is one of very great uncertainty as well as difficulty. Difficulty incites rather than repels a true experimenter, but uncertainty is paralysing.

233. Though, as we have seen, we can give only general and somewhat vague numerical data, there is practical unanimity on the part of experimenters that, *within the limits of elasticity*, Hooke's law is very closely followed. Hence, although it is necessary to measure the elastic coefficients for each specimen of each substance we employ, once that measurement is effected we can trust to it as giving the special qualities of the material through a range of stress which, in glass, steel, etc., is often fairly wide. One excellent example is to be found in the substitution of glass or steel for air or nitrogen in the construction of instruments for measuring hydrostatic pressure.

The first to introduce this principle seems to have been Parrot,[1] whose *Élatéromètre* was merely an ordinary thermometer, with a bulb thick enough to stand great pressure. Keeping it immersed in water at a constant temperature, and applying great pressures, he found that the diminution of capacity of the bulb was almost exactly proportional to the pressure.

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[1] "Expériences de forte Compression sur Divers Corps," *Mém. de l'Académie Impériale des Sciences de St Petersbourg*, 1833.

Instruments working on the same general principle have since been introduced, in ignorance of Parrot's work, by many investigators. Bourdon gauges, aneroid barometers, etc., are merely special though rather complex instances.

Sudden application of pressure produces temperature-changes which affect especially the volume of the liquid contents by means of which the distortion is usually measured. But these instruments (in Parrot's form at least) may be made practically insensible to such changes by the simple expedient of *nearly* filling the bulb (which, for this purpose, should be cylindrical) with a piece of glass tube closed at each end.[1] The mercury in the bulb is thus greatly reduced in quantity, and therefore the temperature effects in the stem are very small, while the instrument is still as sensitive as ever in indicating changes of volume.

The dimensions and thickness of such an instrument, for any special purpose, can be easily calculated from the formulæ of § 183; and the unit of pressure can be determined for it, by a single comparative experiment, with the aid of Amagat's table of compression of air (§ 200).

There is great advantage in using simultaneously two instruments of this kind, in one of which the thickness is considerably greater (in comparison with the diameter) than in the other. For, so long as their indications agree, *both* may be trusted as following Hooke's law very accurately.

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[1] Tait, *Report on the Pressure Errors of the Challenger Thermometers*, 1881.

234. The limit of pressure measurable by means of these instruments depends upon the resistance of a glass or steel tube to crushing by external pressure. From a series of experiments, made for the purpose,[1] Tait has calculated that ordinary lead glass (in the form of a tube closed at each end) gives way when the distortion of the interior layer amounts to a shear of about $1 \pm \frac{1}{280}$, coupled with a compression of about $\frac{1}{600}$. Hence even a very thick tube of such glass cannot resist more than about 14 tons' weight per square inch (2130 atmospheres) of external pressure. No corresponding experiments seem yet to have been made for steel.

235. We now come to the case of bending of a rod or bar. Here we have no such simple problem as in the case of the torsion of a cylinder, and must consequently *assume* the solution as given by mathematical investigation; based, of course, on the principles already explained. This shows us that, so long as the radius of curvature is large in comparison with the thickness of the bar in the plane of bending, the line passing through the centre of inertia of each transverse section, the *elastic central line* as it is called, is bent merely, and not extended nor shortened.

The *flexural rigidity* of the bar, in any plane through the central line, is directly as the couple, in that plane, which is required to produce a given amount of curvature in the central line. Its amount may easily be calculated by means of the following considerations. Let the figure represent a transverse section of the cylinder, C its centre of inertia, CD a line in it perpendicular to the plane of bending, and let the centre of curvature of the bending lie towards E. Then obviously all lines parallel to the axis of the bar on the E-ward side of CD are compressed, all towards the other side extended; each in proportion to its distance from CD and to the curvature. If we contemplate a transverse slice, of small thickness t, we see that its thickness remains unchanged along CD, is diminished on the E-ward side of that line, and increased on the other. The thickness at the small area A becomes $t\left(1+\frac{\mathrm{AB}}{r}\right)$, where r is the radius of bending. This requires a tension $\mathrm{A}\,\frac{\mathrm{AB}}{r}\,m$, where m is Young's modulus. The moment of this about CD is—

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[1] *Proc. R.S.E.*, April 18, 1881.

$$\mathrm{A}.\mathrm{AB}^2.\frac{m}{r}.$$

Hence the sum of all such, *i.e.* the moment of the bending couple, is $\frac{m}{r}$ multiplied by the moment of inertia of the area of the section about CD. (The reversed strain, in the compressed parts, requires the reversed stress. § 177.) Now through C in the plane of the section, there are two principal axes of inertia of the area, in directions at right angles to one another. Hence, except in the cases of "Kinetic Symmetry" of the section (as when it is circular, square, equilateral-triangular, etc.), there are two principal flexural rigidities, a maximum and a minimum, in planes (through the axis)

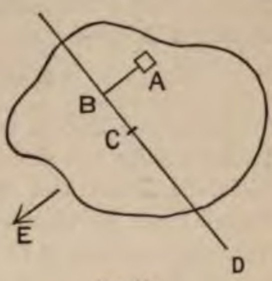

Fig. 26.

perpendicular to one another. If the rigidities in these planes be called R_1 and R_2, the flexural rigidity in a plane (through the central line) inclined at an angle θ to that of R_1 is—

$$R_1 \cos^2.\theta + R_2 \sin^2.\theta.$$

[Compare § 228, in which the corresponding case of torsion-rigidity was shown to depend upon the moment of inertia of the area of the section about the elastic central line. This is the third principal axis of the transverse sectional area at its centre of inertia.]

236. It appears from last section that flexure (within moderate limits) is, practically, as regards any very small portion of the substance, the same thing as longitudinal extension or compression, and thus cannot give us any simple information as to the elastic coefficients of the substance. But it has very important practical applications, and therefore we devote some sections to the more common cases.

The principal moments of inertia of the area of a rectangle, sides $2a$ and $2b$, about axes through its centre and parallel to pairs of sides, are $4a^3b/3$ and $4ab^3/3$. Multiplied by m, they represent the flexural rigidities of a plank in planes parallel to its broader, and to its narrower faces respectively. These rigidities, multiplied by the bending curvature, give the couple required to produce and to maintain the flexure.

237. The *Elastic Curve* of James Bernouli, celebrated in the early days of the differential calculus, is a particular case of the bending of a wire or plank, in which the flexural rigidity in the plane of bending is the same throughout, and a simple stress (§ 128) alone is applied.

The obvious condition is that the curvature at each point is directly proportional to the distance from the line in which the stress acts. For the investigation of the equation of the curve from this condition, and for drawings of its various forms, the reader must be referred to works on *Abstract Dynamics*;[1] but we figure here the special case which corresponds to a stretched uniform wire, of infinite length, with a single kink upon it. This will be referred to in § 289 below.

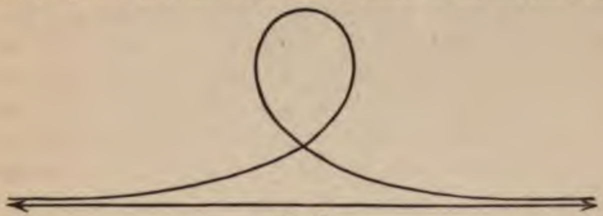

FIG. 27.

The investigation of the bending of planks, variously supported, and under various loads, is a somewhat generalised form of the question of the elastic curve. The principles involved in its solution are simple, and almost obvious; but the mathematical treatment of it would lead us too much out of our course. So would that of the problem of the effect of a couple applied anyhow to one end of a cylindrical or prismatic wire, of any form of section, the other end being fixed. The wire, in such a case, takes generally the form of a circular helix:—involving both flexure and twist. The extreme particular cases are—(1) when the wire is in the plane of the couple, and there is bending only; (2) when the wire is perpendicular to the plane of the couple, and there is twist only.

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[1] See, for instance, *Thomson and Tait's Nat. Phil.*, vol. i. part ii. p. 148.

238. The results hitherto given are all approximate only, and depend upon the radius of bending being large compared with the thickness of the wire or bar in the plane of flexure. Those given in § 228, for torsion, may be applied, under a similar restriction, to cases in which the section of the wire or bar is not circular. The mathematical treatment of the *exact* solution of such problems is of too high an order of difficulty for the present work; but some of its results, alike interesting and important, may be easily understood. A few of them will now be given, but the reader must be referred to the works already cited (§ 227) for a more complete account.

239. Thus in the flexure of a uniform bar into a circular arc, we saw (§ 235) that each fibre is extended or compressed to an amount depending on its original distance from the plane which passed through the centres of inertia of its transverse sections (while it was straight), and perpendicular to the plane of bending. But this involves (§ 177) compression or extension of the transverse section of the fibre, uniform in all directions, and to an amount proportional to the extension or shortening of its length. Hence, if the section of the unbent bar be divided into equal indefinitely small squares, each of these will remain a square after bending. From this we can obtain an approximate idea of the change of shape of the transverse section.

Consider the annexed figure, which represents parts of a series of concentric circles, whose radii increase in a slow *geometrical* ratio, intersected by radii making with one another equal angles such that the arcs into which any one circle is divided are equal to the difference between its radius and that of the succeeding circle.

When the circles and radii are infinitely numerous, all the little intercepted areas are squares. The sides of the squares along CD are obviously greater than those of the squares along AB by quantities proportional to AC. Those of the squares along EF are less than those of the squares along AB by quantities proportional to AE. The figure CDFE must therefore represent the distorted form of the cross section of a beam, originally rectangular, and bent in a plane through OG, and perpendicular to the plane of the figure. The side of the beam which is concave in the plane of flexure is convex in a direction perpendicular to the plane of flexure; that which is convex in the former plane is concave in the latter. The cause is, of course, the transverse swelling of the fibres on the side towards G, the centre of bending, and the diminution of section of those on the other side of the bar. It is sufficiently accurate to assume that AB, which is unchanged in length, was originally midway between the faces of the bar.

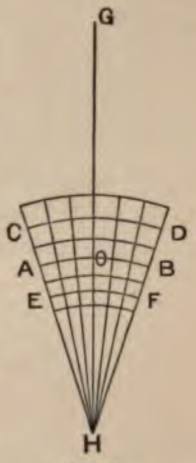

FIG. 28.

If OG be the radius of flexure, the ratio of the extension of one of the fibres which pass through a point of EF to its original length is AE/OG. Its lateral contraction in all directions must therefore be (§ 180)

$$\frac{3k-2n}{2(3k+n)}\,\text{AE/OG}.$$

But it is obviously AE/OH. Hence

$$2(3k+n)\text{OG}=(3k-2n)\text{OH}.$$

Thus the point H is determined, and the approximate solution is complete. A square bar of vulcanised india-rubber shows very clearly the characteristics of this strain.

240. In the case of torsion of a cylinder whose section is not circular, plane transverse sections do not remain plane. The following figure gives de St Venant's result for an elliptic cylinder. It represents the contour lines of the distorted section made by planes perpendicular to the axis. They are equilateral hyperbolas (as in § 88), the common asymptotes being the axes of the section.

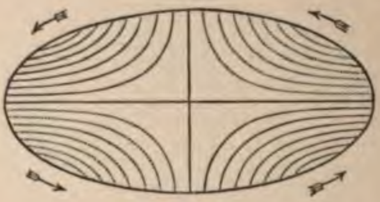

FIG. 29.

The torsion is applied in the positive direction to the end of the cylinder above the paper; and the full lines represent distortion upwards; the dotted, downwards.

241. Coulomb, who first attacked the torsion problem, was led (by an indirect and unsatisfactory process) to the result above (§ 228), viz. that the torsional rigidity is proportional to the moment of inertia of the area of the transverse section about the elastic central line. This is true only in circular cylinders or wires. It gives too large a value for all other forms of section. From de St Venant's paper we extract the following data. The first numbers express the ratio of the *true* torsional rigidity to the estimate by Coulomb's rule. The second numbers

show the ratio of the torsional rigidity to that of a cylinder, of the same sectional area, but circular.

Equilateral Triangle.	Square.
0·600	0·843
0·725	0·883

The torsional rigidity of an elliptic cylinder, a and b being the semi-axes of the transverse section, is

$$\pi n \frac{a^3 b^3}{a^2+b^2}.$$

When $b = a$ we have, of course (as in § 228),

$$\frac{\pi}{2} n a^4.$$

242. From these and like results we are led to see that projecting flanges, which add greatly to the flexural rigidity of a rail or girder, are practically of no use as regards resistance to torsion.

Another of de St Venant's important results is that the places of greatest distortion in twisted prisms are the parts of the boundary *nearest* to the axis.

Near a re-entrant angle in the boundary of the section the stress and strain are usually very much greater than at any other part, whether the stress be such as to produce torsion or bending. Hence the reason for the very important practical rule of always rounding off such angles, when they cannot be entirely dispensed with.

243. Still keeping to statical experiments, we have to consider briefly the *limits of elasticity*.

When a solid is strained beyond a certain amount, which depends not merely on its material but upon its state and the mode of its preparation, one of two things occurs. Either it breaks, and is said to be *brittle*, or it becomes permanently distorted, and is said to be *plastic*.

Different kinds of steel, or the same steel differently tempered, give excellent instances. Some have qualities superior to those of the best iron, others are more brittle than glass. Even in the case of one definite material, the results differ widely from one another according to the duration of the stress, and also according to the way in which it is applied :—*i.e.* by successive large or small instalments, and with longer or shorter intervals between the applications.

244. When a body has been permanently distorted, as, for instance, a copper wire which has received a few hundred twists per foot, it has new limits of elasticity (within which Hooke's law again holds, though with altered coefficients) ; but the elasticity, at all events for distortions of the same kind, is usually of a very curious character, inasmuch as the strain produced by a stress will, in general, no longer be exactly reversed by reversal of the stress. In fact the body has been rendered non-isotropic ; and, so far as this problem has yet been treated (though that does not amount to much), it is of the order of questions which we cannot enter on in this volume.

The limits of elasticity vary so much, even in different specimens of the same material, that no numbers need here be given. Every one who has occasion to take account of these limits must determine them for himself on the materials he is about to employ.

245. A curious fact, showing that elasticity may remain *dormant*, as it were, is exhibited by sheet india-rubber. When it has been wound in strips, under great tension, on a stout copper wire, and has been left in that condition for years, it appears to harden in its state of

strain, and can be peeled off like a piece of unstretched gutta-percha. But, if it be placed in hot water, it almost instantly springs back to its original dimensions. The experiment may be made, but with less perfect results, in a few minutes, by merely putting the strained india-rubber into a mixture of snow and salt.

246. Excellent instances, illustrative of the possibility of arrangements giving peculiar kinds of non-isotropy, are furnished by many manufactured articles, such as woollen or linen cloth, wire-gauze, etc., in which Young's modulus is large for strips cut parallel to the warp or woof, but small for strips cut diagonally. Still more curious is a special kind of wire-gauze in which the meshes are rhombic. Another suggestive instance is a strip formed of wire knotted as in Fig. 30, in which the flexure and torsion rigidities for any bending or twist, and its reverse, are in general markedly different. Similarly a shirt-of-mail made of rings, each three joined as in the first figure (31 above), is perfectly flexible; as in the second figure, nearly rigid.[1]

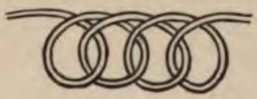

FIG. 30.

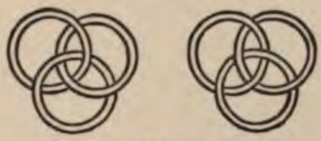

FIG. 31.

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[1] On Knots. *Trans. R.S.E.*, 1877.

247. Kinetic processes for determining coefficients of elasticity are often based upon the pitch of the note given out by a vibrating body. We do not give any of these, as they belong properly to the subject of SOUND. All require an exact determination of pitch, and (except in the very simplest case, that of stretched wires, as those of a pianoforte) require, for their comparison with the other experimental data, higher mathematics than we can introduce here.

248. There is, however, one kinetic process of a very simple character (we have already adverted to it while describing the Cavendish experiment, § 153) by which the rigidity of a substance is determined from torsional vibrations.

The wire to be experimented on is firmly fixed at its upper end, and supports a mass whose weight is sufficient to render it straight, but not so great as to produce any sensible effect on its rigidity. The moment of inertia of this mass may be caused to have any desired value by making the whole into a transverse slice of a hollow circular cylinder of sufficient radius, which can be very accurately turned and centred on a lathe. The wire must be attached to the middle of a light cross bar, so as to lie in the axis of this cylindrical vibrator.

If N represent the torsional rigidity of the wire, l its length, and φ the angle through which the vibrator has been turned, the elastic couple is

$$-\mathrm{N}\frac{\varphi}{l}.$$

The rate at which work is done against the elastic forces is

$$\mathrm{N}\frac{\phi}{l}\phi.$$

But this must be equal to the rate at which the appended mass loses energy of rotation, *i.e.* (§ 135)

$$-\mathrm{I}\,\dot{\phi}\,\ddot{\phi}$$

if I be its moment of inertia. Hence

$$\ddot{\phi}+\frac{\mathrm{N}\phi}{\mathrm{I}l}=0.$$

This shows (§ 72) that the oscillations are of the simple harmonic character, and that the period is

$$2\pi\sqrt{\frac{\mathrm{I}l}{\mathrm{N}}},$$

or, if the wire be of circular section (§ 228),

$$\sqrt{\frac{8\pi\mathrm{I}l}{n\mathrm{R}^4}}.$$

In this expression all the factors are known, with the exception of n, which can therefore be determined.

The chief difficulties in the application of this process are the finding exactly the radius of the wire, and the ensuring that its substance is really isotropic.

249. The solution just given is accurate *only* if all the circumstances have been taken into account. But a very few trials, with wires of different metals, show that the range of vibration diminishes at every oscillation, and with some metals much more rapidly than with others. This cannot, therefore, be wholly due to the resistance of the air. Part of it, at least, is undoubtedly due to the dissipation of energy, by thermal effects of change of form, which occur even when the elasticity is perfect. This, however, is beyond our province, as defined in § 175. But a large part, with metals like zinc much the greater part, is due to internal viscosity.

250. So long as we deal with steel, iron, silver, etc., and keep to torsions well within the limits of elasticity,

the arc of oscillation is found to diminish in simple geometrical progression. This points to a resistance to the motion, partly due to air acting on the suspended mass, partly to thermal effects and to viscosity in the wire itself, but, on the whole, proportional to the rate of motion, *i.e.* the rate of distortion.

Thus the equation of § 248 takes the form

$$\ddot{\phi}+A\dot{\phi}+\frac{N}{Il}\phi=0.$$

The solution of the problem in this case is, therefore, of the nature of that given in § 74 above; and we see that, if the diminution of the arc of oscillation (per vibration) is large, the periodic time will be perceptibly increased. Thus the direct determination of n, by the mode of calculation given in § 248, would necessarily lead to underestimation of its value.

The logarithmic decrement of the arc of vibration gives us κ, the time of vibration gives us ω, and then we have

$$\omega^2+\kappa^2=\frac{N}{Il},$$

whence N, and therefrom n, can be found.

251. All this part of our subject is still very imperfectly worked out. We have already seen (§ 50) that even brittle bodies may be completely changed in form by small but *persistent* forces. And there is no doubt that all elastic recovery in solids is gradual, so that, for instance, in the torsion vibrations which we have just considered, even when there is no sensible viscous resistance, the middle point of the range does not coincide with the original untwisted position of the wire. It is always shifted towards the side to which torsion was first directed, and to a greater extent the longer the wire has been kept twisted before being allowed to

vibrate. With every vibration, however, the middle point of the range creeps slowly back towards the original undisturbed position, but the oscillation usually ceases before this is reached. Still, even after the oscillation has ceased, the wire continues to untwist, more and more slowly, sometimes not even approximately reaching its undisturbed position till hours or even days have passed.

When viscous resistance is considerable these results are usually still more marked; and Lord Kelvin[1] has discovered the very curious additional fact that this molecular friction becomes greatly increased by keeping the wire oscillating for days together. He has pushed this process so far with one of two similar wires that, whereas, in that which had been made to vibrate only a few times, the arc of oscillation became reduced to half in 100 vibrations, the (equal) arc of that whose elasticity had been "fatigued" fell to half after 44 or 45 vibrations only.

252. Some of these phenomena are seen in a still more striking form when we dispense with oscillation. Thus, for example, suppose the wire to be kept twisted through 90° to the right for six hours, then for half an hour 90° to the left, and be then so gradually released that there is no oscillation. When it is left to itself it turns slowly towards the right, gradually undoing part of the effect of the more recent twist, then stops, and twists still more slowly to the left, thus undoing part of the quasi-permanent effect of the earlier twist. Thus the behaviour of such a wire, strictly speaking, is an excessively complex one, depending, as it were, upon its whole previous history; though, of course, the trace left by each stage of its treatment is less marked as the date of that stage is more

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[1] *Proc. R.S.*, 1865.

remote. This subject has of late attracted great attention in Germany, and, under the name *Elastische Nachwirkung*, has been the object of numerous researches by Wiedemann, Kohlrausch, Boltzmann, etc.

253. Clerk-Maxwell[1] has given a sketch of a theory of this peculiar action, from which we quote the following:—

"We know that the molecules of all bodies are in motion. In gases and liquids the motion is such that there is nothing to prevent any molecule from passing from any part of the mass to any other part; but in solids we must suppose that some, at least, of the molecules merely oscillate about a certain mean position, so that, if we consider a certain group of molecules, its configuration is never very different from a certain stable configuration, about which it oscillates.

"This will be the case even when the solid is in a state of strain, provided the amplitude of the oscillations does not exceed a certain limit, but if it exceeds this limit the group does not tend to return to its former configuration, but begins to oscillate about a new configuration of stability, the strain in which is either zero, or at least less than in the original configuration.

"The condition of this breaking up of a configuration must depend partly on the amplitude of the oscillations, and partly on the amount of strain in the original configuration; and we may suppose that different groups of molecules, even in a homogeneous solid, are not in similar circumstances in this respect.

"Thus we may suppose that in a certain number of groups the ordinary agitation of the molecules is liable to accumulate so much that every now and then the configuration of one of the groups breaks up, and this whether it is in a state of strain or not. We may in this case assume that in every second a certain proportion of these groups break up, and assume configurations corresponding to a strain uniform in all directions.

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[1] "Constitution of Bodies," *Ency. Brit.*, ninth edition.

"If all the groups were of this kind, the medium would be a viscous fluid.

"But we may suppose that there are other groups, the configuration of which is so stable that they will not break up under the ordinary agitation of the molecules unless the average strain exceeds a certain limit, and this limit may be different for different systems of these groups.

"Now if such groups of greater stability are disseminated through the substance in such abundance as to build up a solid framework, the substance will be a solid, which will not be permanently deformed except by a stress greater than a certain given stress.

"But if the solid also contains groups of smaller stability and also groups of the first kind which break up of themselves, then when a strain is applied the resistance to it will gradually diminish as the groups of the first kind break up, and this will go on till the stress is reduced to that due to the more permanent groups. If the body is now left to itself, it will not at once return to its original form, but will only do so when the groups of the first kind have broken up so often as to get back to their original state of strain.

"This view of the constitution of a solid, as consisting of groups of molecules some of which are in different circumstances from others, also helps to explain the state of the solid after a permanent deformation has been given to it. In this case some of the less stable groups have broken up and assumed new configurations, but it is quite possible that others, more stable, may still retain their original configurations, so that the form of the body is determined by the equilibrium between these two sets of groups; but if, on account of rise of temperature, increase of moisture, violent vibration, or any other cause, the breaking up of the less stable groups is facilitated, the more stable groups may again assert their sway, and tend to restore the body to the shape it had before its deformation."

254. There remains one specially complex kinetical case of elastic reaction, *i.e.* the effects of *Collision.* According to Newton, the "rules of the congress and reflection of hard bodies" were discovered about the same time by Wren, Wallis, and Huygens. Wallis had the priority, then followed Wren. But Wren "confirmed the truth of the thing" by a pendulum experiment (see Appendix IV.). By "hard bodies" are meant such as rebound from one another with the same *relative* velocity as they had before collision. Newton goes on to describe his own mode of experimenting on the subject, how he allowed for the resistance of the air, etc., and proceeds as follows:—

"By the theory of Wren and Huygens, bodies absolutely hard return one from another with the same velocity with which they meet. But this may be affirmed with more certainty of bodies perfectly elastic. In bodies imperfectly elastic the velocity of the return is to be diminished together with the elastic force; because that force (except when the parts of bodies are bruised by their congress, or suffer some such extension as happens under the strokes of a hammer) is (as far as I can perceive) certain and determined, and makes the bodies to return one from the other with a relative velocity, which is in a given ratio to that relative velocity with which they met. This I tried in balls of wool, made up tightly and strongly compressed . . . the balls always receding one from the other with a relative velocity, which was to the relative velocity with which they met as about 5 to 9. Balls of steel returned with almost the same velocity: those of cork with a velocity something less; but in balls of glass the proportion was about 15 to 16."

Of course results of this kind are confined to moderate relative speeds. The question becomes a very different and vastly more difficult one when very high relative speeds are contemplated. When the relative speed is such as to lead to the breaking of one of the two bodies, we have a problem of at least as high an order of difficulty as that presented by Tenacity. (§ 226.)

255. So far as spherical bodies of small size, and impinging on one another with a moderate relative speed, are concerned, there is yet but little to add to Newton's results. These results, however, were fully confirmed by the careful and instructive experiments of Hodgkinson.[1] Though the difficult mathematical problem of the deformation and elastic rebound of two impinging spheres has been magnificently treated by Hertz,[2] his results are applicable only to very slight distortions.

But recent inquiries have shown that Newton's use of the term "perfectly elastic" is not correct, for two bodies may be perfectly elastic, and yet *not* rebound from one another with the relative velocity of their approach. This happens, in an easily intelligible manner, when a bell or other body capable of vibrations is struck by a hammer.

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[1] *Brit. Ass. Report*, 1834.

[2] *Crelle*, xcii, 1882. See also Schneebeli, *Arch. de Genève*, 1884.

And it is clear that the problem of impact of large masses, where the propagation of distorting stress in each has to be taken account of, will prove a very difficult one.

In modern phraseology the ratio of the relative velocity of recoil to that of collision is called the *Coefficient of Restitution.* It is not directly a coefficient of elasticity, for it depends to some extent upon the sizes and shapes of the impinging bodies, as well as upon the materials of which they consist.

256. It is clear that, so far as direct impact of spheres is concerned (where the whole motion of each of the masses is in one common line), the third law of motion, along with the value of the coefficient of restitution, suffices for the calculation of the entire circumstances of the motion after impact.

For if M, M′, be the masses of the spheres, v, v' and v_1, v_1' their respective speeds before and after impact, and e the coefficient of restitution, the third law gives

$$Mv_1 + M'v_1' = Mv + M'v'$$

while the elastic property gives

$$v_1' - v_1 = e(v - v');$$

so that v_1 and v_1' are fully determined.

This part of the subject will be found fully discussed in most treatises on Dynamics. For illustrations of cases in which, even with perfect elasticity, the coefficient of restitution is necessarily less than unity, the reader may consult Thomson and Tait's *Natural Philosophy*, § 304.

257. In the case of violent impact between bodies of small dimensions, as in golf or cricket, the mutual action (in the first sense of Newton's Third Law, § 128) usually

increases faster than in direct proportion to the deformation, measured by the approach towards one another, after contact, of the undistorted parts of the two bodies. This will be the case even while Hooke's Law holds, because the greater the deformation the more extensive usually are the parts deformed. Thus (§ 71) the duration of impact is *less* the greater is the relative speed ; so long at least as no permanent distortion is produced.

The duration of compression is obviously greater than the time in which a point, moving with the initial relative speed, would pass over a space equal to the deformation (measured as above). But it is less than twice as great. For, since the mutual action increases faster than does the deformation, and is such as to *retard* the relative speed, its time-average value during the compression must be greater than its space-average. That is

$$\frac{MV}{T} > \frac{MV^2}{2D}$$

where T is the time of compression, and D the normal deformation. This gives

$$T < \frac{2D}{V},$$

which is the statement above. The period of recovery is longer than that of compression in the ratio 1 : e, approximately.

Some notion of the duration of impact may be obtained from the following experimental results.[1] A block of hard wood, weighing $5\frac{1}{2}$ lbs., fell from a height of 4 feet on a cylinder $1\frac{1}{4}$ in. in diameter and $1\frac{1}{4}$ in. long, the lower half of which was imbedded in a large mass of lead resting on the ground.

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[1] *Proc. R.S.E.*, 1889–90, p. 192 ; *Trans. R.S.E.*, 1891, 1893.

Material of Cylinder.	Distortion.	Time of Impact.
	mm.	s.
Vulcanised India-Rubber	11·5	0·0077
Vulcanite	2·3	0·0014
Cork	19·0	0·0166
Plane-tree	1·9	0·0018

It appears from the experiments that the elastic force, called into play by the distortion, varies nearly in the sesquiplicate ratio of the amount by which the cylinder is shortened. It is very curious to find that this result, deduced from experiments in which the elastic body is violently strained *throughout* (being virtually struck at both ends simultaneously) is in exact accordance with the theoretical result of Hertz, in which the impinging bodies were supposed to be perfectly free except in the region of their mutual contact, and the whole distortion was assumed to be very small.

A needle-point, attached to the block, recorded its motion on a rapidly revolving disc of glass, thinly covered with fine printer's ink; and time was measured by a simultaneous record made by a tuning fork maintained in vibration by a periodic electric current. When a golf-ball was substituted for the cylinder, the time of impact was about $0^{\text{s}}{\cdot}004$.

Thus a golf-ball (since its mass is about 0·1 lb. only) has rebounded from the club before it has described a space equal to half its radius:—and the whole time of impact is of the order of one ten-thousandth of a second.

CHAPTER XII.

COHESION AND CAPILLARITY.

258. A SOMEWHAT pedantic nomenclature has introduced the terms *Cohesion* and *Adhesion* in senses distinct from one another. Thus contiguous parts of a piece of glass, or of a drop of water hanging from it, are said to cohere, while the water is said to adhere to the glass. Such pedantry usually tends to produce confusion, as will be seen at once if we try to state in its language how the parts of a lump of granite, or of a drop of mixed alcohol and water, are kept together. We will therefore use, indiscriminately, whichever of the words happens to present itself when we require one of them.

We have already referred to the molecular forces which are practically alone efficient in keeping together the particles of a solid of moderate dimensions (§ 167), and to the resolidification (by pressure) of powdered graphite (§ 53). The one characteristic of these forces, and that which specially contrasts them with gravitation, is that *they are insensible at sensible distances.* We have studied the elasticity of fluids and of solids, and have also made some remarks on the tenacity of solids (§ 226). A few other instances of cohesion between the particles of solids may now be noticed, but the subject is one on which no *exact* information can be expected in the present state of science.

Two masses of marble, on each of which a true plane surface has been worked, will, when these surfaces are brought firmly together, even *in vacuo*, adhere so as to overcome the weight of either (unless it be great in comparison with the area of contact), so that one remains suspended from the other. Barton, early in the century, made a set of cubes of copper whose sides were so very true that when a dozen of them were piled on one another the whole series adhered together when the upper one was lifted. If a small plane surface be scraped bright on each of two pieces of lead, and these be pressed together (with a slight screwing motion) they adhere almost as if they formed one mass. The processes of gilding, silvering, nickelising, etc., and their results, are known to all. So are the properties of lime, glue, and other cements, all depending on the molecular forces in and between solids.

259. Nor are we in a better position when we seek, by what used to be considered a direct mode of measurement, the force of adhesion between a solid and a liquid. In the great majority of cases, the liquid wets the solid: so that when we suspend a plate of the solid horizontally from one scale-pan of a balance, and try what amount of weights we must put into the opposite pan so as just to detach the plate from a liquid surface, the liquid itself is usually divided, not directly separated from the solid. Such experiments, besides being very tedious and difficult, lead to no results of the kind sought (see § 287). We have seen (§ 219) that the adhesion of water to glass is, at least, 800 lbs. weight per square inch. But a force of about 60 grains' weight only is required to draw a square

inch of glass (wetted) from the surface of water; while, if the plate be carefully cleaned and dried, only about three times as great a force is required to separate it from clean mercury. When a square inch of amalgamated zinc is used it requires more than 500 grains' weight to remove it from mercury. Here, however, as in the case of glass and water, it is the liquid which is divided (§§ 275, 294).

260. The *Internal* (or molecular) *Cohesion* of a liquid is suggested at once from analogy by the tenacity of a solid. Its amount is large (§ 219), but in consequence of the mobility of liquids its direct measurement presents experimental difficulties apparently insuperable. It will be seen later that it can be calculated from the surface-tension, though the value thus obtained will be too small. It must be carefully distinguished from what has been called the *Internal Pressure* of a liquid; a conception not very easy to define, because somewhat difficult to grasp.

In some statical theories of molecular action, especially that of Laplace, one of the most striking deductions is that there must be a very great internal pressure in every liquid mass:—a pressure wholly independent of the form and size of the bounding surface. This is usually known as "Laplace's K." Laplace's own estimate of its value in water is given (with the caution "une aussi prodigieuse valeur ne peut pas être admise avec vraisemblance") as the weight of the water which would fill a tube of unit section whose length is 10,000 times the distance of the earth from the sun: *i.e.* something like 10^{12} tons weight per square inch.[1] This was based on the corpuscular theory of light, the numerical data being the refractive index of water and the speed of light.

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[1] *Méc. Céleste,* livre x., Supplément à la Théorie de l'Action Capillaire.

No doubt the idea of molecular force was introduced to explain, among other things, the crowding of the particles together. But, as the particles are not supposed to be jammed together, it is clear that they must be regarded as acting repulsively when they are very near one another. Thus the first layers of particles, on either side of any imagined plane drawn in the liquid, may be considered as repelling one another, while the constituents of the second, third, &c., layers on either side of the plane attract these, and one another, across it. If this attraction be regarded as producing a pressure-stress $+K$, why should we not speak of a simultaneous pressure-stress $-K$ due to the repulsion? Thus, from the statical point of view, there would be no molecular pressure at all:—though there must, of course, be compression sufficient to bring the requisite repulsion into play. This compression must obviously increase, from the surface inwards, but only through a depth of the order of the molecular range. For the rest of the mass it is constant. Thus there is a surface film in which the state of the liquid is different from that throughout the rest.

The case presents a somewhat different aspect when we look at it from the kinetic point of view:—for then we have, in place of statical molecular repulsion, the impulse-energy of moving particles, which produces a stress sufficient to balance K.

When external pressure is applied, the values of the balancing stresses will (on either of the above theories) be increased, because of the further compression produced. But the repulsive stress must then exceed the attractive by the amount of the external pressure; and to that extent there will be what is ordinarily meant by hydrostatic pressure, throughout the liquid.

[It is possible that the quantity Π, in the empirical formula (§§ 216, 217) for the average compressibility of a liquid for the first p atmospheres :—viz.

$$\frac{v_0 - v}{p v_0} = \frac{e}{\Pi + p},$$

may represent the pressure-stress K ; for the formula may be written as

$$(\Pi + p)(v - a) = \Pi(v_0 - a) ;$$

where

$$a = v_0(1 - e)$$

represents the (theoretical) volume under infinite pressure. By the remark above, Π increases with the compression. This meets exactly the slight defects of the formula, pointed out in § 217.]

261. We now come to phenomena in which accurate measurements are in general possible. These are the phenomena due to the *Surface-Tension* of liquids. We owe the idea to Segner (1751), but its development and application are due mainly to Young. The more recent theoretical advances in the subject were made chiefly by Laplace and Gauss. [For a sketch of the history of this subject the reader is referred to Clerk-Maxwell's article, "Capillary Action," in the ninth edition of the *Ency. Brit.*]

262. As soon as we recognise, as a fact, the extremely short distance at which these powerful molecular forces are sensible, we see that there must be an essential difference in state between parts of a liquid close to the surface and others in the interior of the mass. For if we describe, round any particle of the liquid as centre, a sphere whose radius is the utmost range at which the molecular forces are sensible, the only parts of the liquid which act directly on that particle are those contained

within the sphere. So long as the sphere lies wholly within the liquid the forces on the particle must obviously balance one another. [Of course it must be approximately so, unless the least distance from particle to particle is comparable with the radius of the sphere. We know of no liquid for which this is the case.] But when part of the sphere lies outside the liquid surface, *i.e.* when the distance of the particle from the surface is less than the range of the molecular forces, we can no longer make this assertion.

Hence we should expect to find peculiarities in the surface-film whose thickness is approximately equal to this molecular range. [The compressibility is, happily, small in liquids, otherwise the reasoning above would lead to the result that the "peculiarities" should extend to a distance from the surface somewhat greater than the radius of the sphere of action of the molecular forces.] We must appeal to experiment or observation to find their nature in each particular case. And here, as we shall soon see, a multitude of well-known facts comes at once to our assistance. But we must first examine, after Gauss, the theoretical conditions a little more closely.

263. An important theorem of Dynamics is that, for stable equilibrium of a system, the potential energy of the whole must be a minimum. It is easy to see from the considerations given in last section that, *so long as we consider molecular forces alone*, the amount of energy of the liquid mass can vary only with the extent of the surface, but we may formally prove it as follows.

Let the energy be e_0 per unit mass of the interior liquid, and e per unit mass for a layer of the skin, of surface S, thickness t, and density ρ. Then, if M be the whole mass of the liquid, and E its whole potential energy, we have, by summing the energy of the interior mass and of the successive layers of skin (which may be treated as having all practically the same superficies so long as their curvature is finite, in consequence of the shortness of range of the molecular forces),

$$\begin{aligned} \mathrm{E} &= (\mathrm{M} - \mathrm{S}.\ \Sigma t\rho)e_0 + \mathrm{S}.\ \Sigma t\rho e \\ &= \mathrm{M}e_0 + \mathrm{S}.\ \Sigma t\rho(e - e_0). \end{aligned}$$

Thus, in consequence of this property of the skin, the whole energy is increased or diminished by a quantity which is directly proportional to S. The multiplier of S depends upon the nature and the temperature of the liquid, and on the nature of the substance which is in contact with its free surface, only.

Hence, when e is greater than e_0 everywhere throughout the skin, as we find happens when a water surface is exposed to air, S tends to the smallest value compatible with the conditions. But when e is less than e_0, as when water is in contact with glass, S tends to take as large a value as possible. If part be exposed to air, and part be in contact with glass or other substance, the final result is more complex, but involves the same principles.

It is obvious that precisely similar reasoning may be applied to the case of two liquids in contact, even after diffusion has gone on for a little, so long in fact as there remains a sensible difference between the energy per unit mass in the common skin and in either of the liquids. (See §§ 292, 300.) In such a case, however, the resulting changes of form must necessarily take place more slowly than when a single liquid is exposed to air, as the inertia of the whole system has to be overcome.

264. Still keeping to the theoretical view of the subject, let us consider what is implied in the tendency

of the liquid surface to become as small as possible. It must behave, only in an incomparably more perfect manner, like an elastic membrane (such as a sheet of india-rubber), which has been stretched by equal tensions in all directions. But, while the tension of such a membrane becomes less as it is permitted to shrink more and more, the liquid film has still as great a tendency to shrink, however small its surface may have become. Thus it must be under a definite *Surface-Tension.*

If T be this tension across a line of unit-length on such a liquid surface, it is easy to see that the work required to stretch a rectangle, whose sides are a and b, into another whose sides are a and $b+l$, is

$$\mathrm{T}a.l=\mathrm{T}.al$$

i.e. T multiplied by the increase of area. This quantity T must, therefore, be the multiplier of S in the expression (of last section) for the whole energy of a mass of liquid.

265. When the liquid is drawn out into a film, as in blowing a soap-bubble, the tension of this film is practically 2T; so long, at least, as the whole thickness of the film is greater than twice the molecular range. For it may be regarded as consisting of a layer of interior water, with two surface-skins.

266. We now come to the observed facts which are to be compared with these indications of theory. But, first, we assume the mathematical theorem that the sphere is, of all surfaces, that which, with a given content, has the smallest superficies.

Whenever a drop of liquid is left free from all but its own molecular forces, we find it assumes a perfectly spherical form. By far the most rigorous proof of this is afforded by the rainbow. Exceedingly slight deviations from perfect sphericity of the falling drops would suffice entirely to alter the character of this phenomenon. Greater deviations would altogether prevent its occurrence.

The manufacture of small-shot, in which it is important that each particle should be truly spherical, is another good example. A shot-tower, as it is called, is merely a gigantic shower-bath, where the liquid employed is melted lead, which is slightly alloyed, mainly for the purpose of making it less viscous while liquid. The majority of the falling drops solidify in forms very nearly spherical before they reach the water-bath which is employed to break their fall.

The rounding off of the sharp edges of a broken piece of sealing wax, as soon as it melts in a flame, is another example; and it was in consequence of the almost perfect sphericity of the little bead, formed on the end of a glass fibre which is held in a flame for a short time, that such beads were used by the early microscopists as single magnifiers. By the use of these, Leuwenhoeck and others anticipated many of the results which are now obtained by means of the splendid achromatic object-glasses of modern compound microscopes.

267. An ingenious method of guarding a liquid from the action of any but molecular forces was devised by Plateau. He simply placed a mass of oil in a mixture of alcohol and water of the same density as the oil. Here the mass assumes a perfectly spherical form. When this arrangement is altered by evaporation, the mixture becomes denser from the top downwards. The globe of oil becomes flattened, because its lower parts tend to rise and its upper portions to sink, being immersed respectively in parts of the liquid denser and less dense than the oil. This mass of oil can be made to fulfil definite boundary conditions, by bringing into contact with it

various frames of wire, etc., all thoroughly oiled beforehand.

268. If a large drop of water be laid on a clean glass plate, it spreads itself over a considerable surface. [Theoretically, it should wet the whole surface.] Now let ether vapour, which is heavier than air, be poured upon the middle of the water surface; or let it be touched there by a glass rod moistened with alcohol; or even hold the point of a red-hot poker close to it. In all these cases the effect is to reduce, locally, the surface-tension, so that, as it were, a weak part of the surface-film is produced, and this is pulled out over the surface by the greater contractile tension of the unaffected parts of the skin. The water, in fact, often retreats on all sides from the affected part, leaving the central portion of the glass uncovered. The effect is precisely similar to that which is produced in a stretched sheet of india-rubber where one part is either thinner than the rest, or has been slightly heated by a flame.

The surface-tension of a drop of mercury is greatly altered when it is (electrically) "polarised." Remarkable phenomena of this kind were described by Strethill Wright in 1860.[1] Lippmann has employed this surface effect in the construction of a very sensitive electrometer.

269. The phenomenon called the "tears of strong wine," first explained by J. Thomson, is another example. When the sides of a drinking glass have been moistened with strong wine, we observe that the liquid film soon becomes corrugated. The ridges are formed of the portions from which the greatest amount of alcohol has evaporated, and which, therefore, have the greatest surface-tension. As these slide, by gravity, down the

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[1] *Phil. Mag.*, xix. p. 129.

sides, we see them now and then stop, and even *retract*, when they come to a part where there is more alcohol, and therefore less surface-tension.

By far the best example, however, is furnished by some of the less viscid oils. A few drops, let fall on the surface of a quiet pool, seem almost to flash out over the surface, showing in the most brilliant manner the interference colours of thin plates.

Another striking instance of the effects of surface-tension is furnished by a piece of camphor when placed on water. It usually dissolves more rapidly at some one place than at others, thus relatively weakening the surface-tension of the water in that place, and is consequently dragged about with considerable rapidity, and in the most capricious manner. Similar results, but of a more complex and often much more violent nature, are obtained with a pellet of potassium or of sodium.

270. It is the same when we deal with a double skin, as in a soap bubble. When a soap-film is lifted by the mouth of a glass funnel previously wetted throughout its interior with the solution, it rapidly runs up the cone, and fixes itself finally at the place where the area of the cross section is least.

Van der Mensbrugghe has devised a very beautiful and instructive experiment of this kind, which it is easy to repeat. He lays on a soap-film, lifted by a large wire ring, a short endless silk thread, thoroughly wetted with the soap solution. As soon as the film is broken *inside* the coil of the thread, the thread is stretched out into an exact circle which bounds the hole in the film. The rupture is easily effected by touching the film with a hot wire, or a lucifer match just blown out. [The circle is the figure which, with a given perimeter, has the greatest area.

Thus the film has the smallest area, subject to the conditions.]

When a soap bubble is blown at the mouth of a funnel, and the neck is left open, we see it shrink faster and faster, expelling the contained air, which is thus proved to be at greater than atmospheric pressure. Faraday succeeded in blowing out a candle by the air thus expelled.

The soap-glycerine solution, invented by Plateau, gives films which last for hours together, enabling us to study the phenomena of surface-tension even more simply and accurately than can be done by the help of his earlier method (§ 267). His great work[1] on these subjects forms a storehouse of interesting and important experiments, all the more remarkable that they were devised by one who had the misfortune of being permanently disabled from seeing them. The reader must be referred to these volumes for other examples than the few for which we can find room.

271. It is very instructive to observe the mode in which many problems, some of extreme mathematical difficulty, are at once solved practically by experiments with these soap-films.

The whole physical part of the phenomena depends upon the fact that *the film takes the form of smallest superficies consistent with the conditions.*

When the film is exposed to equal pressure on its sides, *i.e.* when no air is anywhere enclosed by it, it may be made up of portions which are individually plane; in which case the problem, though possibly complex, is comparatively easy. But this is an exceptional case; for, even with equal pressures, the film is usually curved, and it must always be curved when the pressures on its sides are different.

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[1] *Statique expérimentale et théorique des Liquides soumis aux Seules Forces Moléculaires.* Paris, 1873.

272. Here it becomes necessary to consider the curvature of the film, and the way in which it depends upon the pressures to which the sides are exposed. A very simple investigation gives us all we require in this matter. Suppose a tape or band, of unit breadth, while under tension T, be wrapped transversely round a cylinder of radius R, and let p be the pressure which it produces on unit surface of the cylinder. Consider a very small portion of its length, AB, subtending an angle θ at the centre, O, of the cylinder. This portion of the band is kept in equilibrium by the tension, T, at its ends, and the reaction, p. AB, of the cylindrical surface. Resolving these forces in the direction OC, bisecting the angle AOB, we have

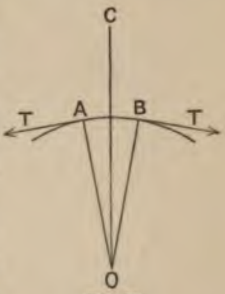

FIG. 32.

$$2\mathrm{T}\sin\frac{\theta}{2}=p\,.\,\mathrm{AB}=p\,.\,\mathrm{R}\theta\,;$$

which, when θ is very small, becomes

$$\mathrm{T}=p\,\mathrm{R},\text{ or }p=\frac{\mathrm{T}}{\mathrm{R}}.$$

Thus the band requires, for its support in the cylindrical form, an excess of pressure on the concave, over that on the convex side, amounting to $\frac{\mathrm{T}}{\mathrm{R}}$ per unit of surface.

273. In the case of a soap-film, or of the surface-film of a liquid generally, there may be simultaneous curvatures in two planes at right angles to one another and to the tangent

plane. The effects of these are to be simply superposed, as they are independent. Let R_1 be the radius of the second curvature, then, as the film exerts equal tension in all directions, the difference of pressures on its sides is, per square unit,

$$T\left(\frac{1}{R}+\frac{1}{R_1}\right).$$

This expression must be doubled in the case of a soap-bubble, for (§ 265) it has *two* surface-films.

This formula may easily be obtained in another way, viz., by expressing the work done during an infinitesimal normal displacement of each point of the film :—*first*, as the product of the difference of external and internal pressure into the increase of contained volume, and *second* as the product of the surface-tension into the increase of surface of the film. This, however, we leave to the reader. He will easily find that if t be the normal displacement of the element, dS, of surface, and p the difference of pressures, we have

$$\int t dS\left(p-T(\frac{1}{R}+\frac{1}{R_1})\right)=0,$$

whatever be the value of t, the integral being extended over the whole surface.

By a well-known geometrical theorem, due to Euler, the quantity multiplied by T, *i.e.* the sum of the curvatures in two planes at right angles to each other, and both passing through the normal to a surface at a particular point, is independent of the aspects of these planes. Hence it is convenient to choose R and R_1 as the *principal* radii of curvature of the film.

When, as a purely mathematical problem, we seek the characteristic of the surfaces of least area which satisfy given boundary conditions, we are led to the condition that *the sum of the curvatures at any point is constant.* This agrees with the physical result.

274. Thus, when a soap-film is exposed to equal pressures on its two sides, it must satisfy the given boundary conditions, and possess the further property that, at every point of its surface,

$$\frac{1}{R}+\frac{1}{R_1}=0;$$

i.e. whatever be its curvature in any normal section, it must have an equal and opposite curvature in the normal section perpendicular to the first.

Such must, therefore, if we neglect the (very slight) disturbing effects of gravity, be the form of a soap-film exposed on both sides to the air. Thus if we lift such a film on a flat loop of wire it assumes a plane surface; but, by bending the boundary, we can make it assume forms of marked curvature. In all its forms, however, the sum of the curvatures at each point is *nil.* And the same is the case, however ramified, linked, or knotted the wire frame may be, provided only that there is no air *imprisoned* at any place.

275. If we imprison a quantity of air by the film, as, for instance, by forming it between the rims of two equal funnels, and closing the neck of each with a finger, we have in general different pressures outside and inside; and then we have (§ 265)

$$\frac{p}{2T}=\frac{1}{R}+\frac{1}{R_1}$$

where p is the constant difference of pressures. By altering the relative position of the funnels, as by shifting one sidewise out of the line of symmetry, or by making it rotate (otherwise than about its axis of symmetry), we can throw the film into extraordinary shapes; all of them, however, possessing the fundamental property of constant sum of the curvatures at each point. But we content ourselves with a brief notice of the results of gradually withdrawing the funnels from one another, while keeping their axes of symmetry in one line.

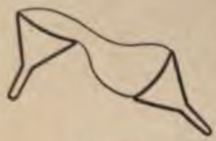

FIG. 33.

Thus we may begin with the film as a quasi-spherical, or even spherical surface, having both its curvatures moderate (Fig. 1). As we withdraw the funnels from one another the longitudinal curvature diminishes, and the transverse increases to the same amount, till at last the longitudinal curvature vanishes altogether, and the film becomes cylindrical (Fig. 2). Still further separating them, the film takes an hour-glass form as in Fig. 3, where the increasing curvature of the transverse section is now balanced by a gradually increasing *negative* curvature in the longitudinal section. At a certain limit this state of the film becomes unstable, and the positive

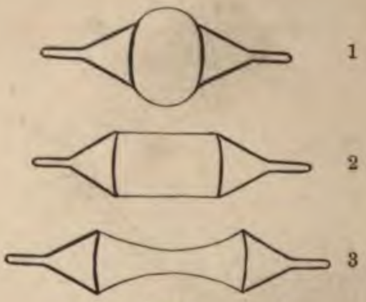

FIG. 34.

and negative curvatures near the middle both rapidly increase, till the walls at that part collapse into a mere neck of water, which is ruptured, and leaves a protuberant film on each of the funnels. By a little dexterous manipulation these may easily be made to reunite into the original form.

276. The facts we have just described show us the nature of the process by which a complete soap-bubble is detached from a funnel, always leaving a film on the funnel ready to produce a second bubble. This process can easily be studied by *completing* the blowing of the bubble with coal-gas, after it has been commenced with air, and watching it detach itself in virtue of the lightness of its contents.

Even so dense a liquid as mercury can be formed into a bubble. We have merely to shake a glass bottle *filled* with water and clean mercury. The bubbles which form on the mercury (often detached) are full of water. Sometimes we see others coming up from the interior of the mercury. These are water-skins full of mercury.

277. When two complete soap-bubbles are made to unite, the tendency of the liquid film is to contract, that of the (compressed) air inside is to expand. It becomes a curious question to find which of these actually occurs.

Let their radii, when separate, be R and R_1, and let them form, when united, a bubble of radius r. Then, if Π be the atmospheric pressure, the original pressures in the bubbles were

$$\Pi + \frac{4T}{R} \text{ and } \Pi + \frac{4T}{R_1};$$

while that in the joint bubble is

$$\Pi + \frac{4T}{r}.$$

By Boyle's Law the densities are as the pressures. Hence, expressing that no air is lost, we have

$$R^3\left(\Pi+\frac{4T}{R}\right)+R_1^3\left(\Pi+\frac{4T}{R_1}\right)=r^3\left(\Pi+\frac{4T}{r}\right),$$

or

$$\Pi(R^3+R_1^3-r^3)+4T(R^2+R_1^2-r^2)=0.$$

If V be the diminution of the whole volume occupied by the air, S that of the whole surface of the liquid film, this condition gives at once

$$3\Pi V+4TS=0.$$

As Π and T are both essentially positive, this condition shows that V and S must have *opposite* signs. Hence *both* tendencies are gratified, the surface, as a whole, shrinks, and the contained air, as a whole, increases in volume, simultaneously. But the work done by the expanding gas is only about two-thirds of that done by the contracting film.

It is worthy of notice that, as is easily proved, the air in a soap-bubble of any finite radius would, at atmospheric pressure, fill a sphere of radius greater than before by the constant quantity $4T/3\Pi$.

278. As a practical illustration of the use of these formulæ, let us apply them to a stationary steam-boiler of the usual cylindrical form, with the ends portions of spheres. If R be the radius of the cylinder, R_1 that of each end, and P the excess of internal over external pressure, the tension is

Across a generating line, RP,

Parallel to a generating line, $\frac{\pi R^2 P}{2\pi R}=\frac{1}{2}RP$,

Across any line on the end, $\frac{1}{2}R_1P$.

Thus, if the boiler-plate be equally tenacious in all directions, there is no danger of the ends being blown off, for the boiler will rather tear along a generating line. And, to make the ends as strong as the sides, they require only half the curvature.

Thus, also, we see why stout glass tubes, if of small enough bore, are capable of resisting very great internal pressure, when, as in Andrews' experiments (§ 198) on carbonic acid, they are exposed only to atmospheric pressure outside.

In what precedes we have neglected the weight of the soap-film, and have consequently taken its tension as being constant throughout. But a moment's consideration of the equilibrium of a plane vertical film shows that the tension must increase from below upwards. This gives an immediate explanation of the difficulty presented by the fact that bubbles cannot be blown with pure water, though its surface-tension is much greater than that of a soap-solution. The soap-solution is, as Marangoni has pointed out, an excessively heterogeneous liquid, and (within limits) can and does adjust its surface-tension to the value required at each point. The slowness with which the film becomes gradually thinner, so as to display in succession the various interference colours of thin plates, is to be ascribed to the viscosity of the liquid.[1]

279. We are now prepared to consider the phenomena properly called *Capillary*, as having been detected in tubes of very fine bore.

When clean glass tubes, each open at both ends, are partially immersed in a dish of water, we observe that (in *apparent* deviation from the hydrostatic laws, § 189) the water rises in each to a higher level than that at which it stands outside. Also we notice that this rise is greater the finer the bore of the tube. The cut shows the phenomenon in section.

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[1] Lord Rayleigh "On the Superficial Viscosity of Water," *Proc. R.S.*, 1890.

Perform the same experiments with mercury instead of water, and we find that the liquid stands at lower levels inside than outside each tube, and that this depression is greater the finer the bore of the tube. Turn the above cut upside down, and it will correspond to this effect.

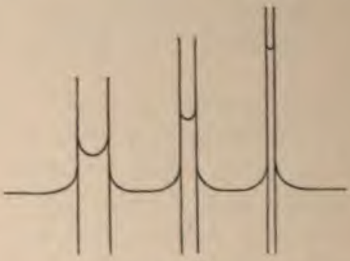

FIG. 35.

280. But a closer inspection at once shows the *immediate* cause of the phenomena. The water surface inside each tube is always concave outwards, that of the mercury convex; and the curvature of either is greater the finer is the bore of the tube.

Remember the surface-tension of the liquid, and the consequent excess of pressure on the concave side, over that on the convex side, which is necessary (§ 272) for its equilibrium, and we see at once that the water immediately under the surface-film must have a *less* pressure than that of the atmosphere to which its concave side is exposed. Thus, hydrostatically (§ 189), it belongs to a higher level than the undisturbed water, whose surface is plane, and the pressure in which (immediately under the surface) is equal to the atmospheric pressure.

[Laplace's K (§§ 260), or whatever may take its place, does not affect this conclusion; for its value is the same at all points of the liquid which are not in the surface-skin].

As the surface curvature is greater in the finer tubes, so the higher rise of water in these is a direct hydrostatic consequence of the greater relief of pressure.

The convexity of the mercury surface, on the other hand, requires immediately under the film a pressure exceeding that of the atmosphere by an amount proportional to the sum of its curvatures. Thus we see why the mercury stands at a lower level in the tube than outside it.

281. It only remains that we should account for the concavity of the water surface, and the convexity of that of the mercury.

In the annexed sections of a concave and of a convex surface, in which a tangent, BA, is drawn to the liquid film, where it meets the side of the tube at B, the angle ABC of the wedge of liquid is obviously less than a right angle for the concave surface, and greater than a right angle for the convex. Hence the problem is reduced to the determination of this angle, called the *Angle of Contact*.

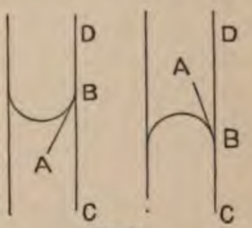

FIG. 36.

That this angle must have a definite value for each liquid, in contact with each particular solid, appears at once from the consideration that, in the *immediate* neighbourhood of B, the gravitational or other external forces, acting on a very small portion of the liquid, are incomparably less intense than the molecular tensions. Hence the equilibrium of that portion (tangentially to the solid) will depend upon the surface-tensions along BA, BC, BD alone. The directions of two of these, and the magnitudes of all three, are determinate, whatever two fluids (even when one is gaseous) are in contact with each other and with the solid (§ 263). BA, therefore, will ultimately assume such a direction that the surface-tension along it will, when resolved in CD, just balance the difference between the tensions in BD and BC. Hence, if that in BD is the greater, the angle of contingence will be acute; if that in BC be the greater, it will be obtuse.

282. In the case of mercury and clean glass, exposed to air, the angle of contact is

140° (Young), 135° (Gay-Lussac), 128° 52′ (Quincke), 132° 2′ (Bashforth).

With water and clean glass in air the angle vanishes entirely:—in fact, of the three tensions, that in BD exceeds the sum of the other two; but when the glass is not clean it may reach (and even surpass) 90°. When it is exactly 90° there is no curvature of the water surface inside the capillary tube, and it therefore stands at the level of the undisturbed water outside.[1]

283. We may now complete the explanation of the

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[1] One of Gay-Lussac's ingenious methods for determining the angle of contact when it is *finite* must be at least indicated here. If the liquid be introduced gradually into a small glass sphere (from below) there will be one position in which its surface is throughout *plane*. By determining this position the angle can be at once calculated. [A plane glass plate dipped into mercury, and inclined until the liquid *under* it is flat throughout, would give an excellent determination.]

behaviour of a liquid in a capillary tube as follows :—When the rise (or depression) exceeds several diameters of the tube, the curvature is practically the same over the whole free surface, which is therefore approximately spherical. In mercury, because of the finite angle of contact, it forms a segment less than a hemisphere; in water it is a complete hemisphere.

In the former case the radius is directly proportional to that of the tube, in the latter it is equal to it. In both cases, therefore, the relief or the increase of pressure, and consequently the rise or depression of the liquid, is inversely as the radius of the tube. This agrees with the (long-known) results of experiment.

284. We may make, in a very simple manner, due to Dr. Jurin, a calculation of the capillary elevation, which is applicable to wider tubes than those spoken of in last section. Suppose the radius of the tube to be r, ρ the density of the liquid, α its angle of contact, T the tension of the surface-film, and h the mean height to which it is elevated. [This mean height is taken such that the volume of the liquid actually raised would, if the surface were not curved, fill the length h of the tube.] Then the vertical component of the whole tension round the edge of the film is obviously

$$2\pi r \mathrm{T} \cos \alpha.$$

But this supports the weight

$$\pi r^2 h \rho g$$

of liquid, (virtually) filling a length h of the tube. Equating these quantities we obtain, after reduction,

$$h = \frac{2\mathrm{T} \cos \alpha}{r g \rho}.$$

When $\alpha > \frac{\pi}{2}$, h is negative, and the liquid is depressed.

All the quantities here are easy to measure except T and α. Hence, if α can be found by a separate process, T is at once determined. In the case of water in clean glass we have $\cos \alpha = 1$, so that the above relation gives T directly.

285. The following values of T are given by Quincke. Each datum in the table belongs to the film at the common surface of the substances whose names are in the same line and column with it.

	Air.	Water.	Mercury.
Water	81	—	418
Mercury	540	418	—
Alcohol	25·5	—	399

The unit here is one dyne per (linear) centimètre. To reduce to grains' weight per inch divide by 25. Thus we may easily calculate, from the formula of last section, that water rises a little more than half an inch in a glass tube whose bore is $\frac{1}{10}$th inch in diameter.

286. In the *Atmometer*, which is merely a ball of unglazed clay luted to a glass stem, the whole filled with water and inverted in a vessel of mercury, not only is the reduction of pressure by the fine concave surfaces of water in the pores sufficient to keep a column of 3 or 4 feet of water supported, but, as evaporation proceeds, mercury rises to take the place of the water, sometimes to 23 inches or more. The process has not, so far as we know, been pushed to its limits. Thus the widest of these pores can sustain (virtually) a column of some 26 feet of water. It is easy to put the Atmometer directly into this condition, and the consequent great concavity of the surface of the water in each pore renders it eminently fit (§ 291) as a nucleus for the deposition of vapour.[1]

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[1] *Proc. R.S.E.*, February 16, 1885.

287. The data of § 285 enable us easily to calculate the force with which a boy's "sucker" is pressed against a stone. Suppose we have two plates of glass, 6 inches square, with a film of water between them whose thickness is $\frac{1}{200}$th of an inch. The force required to pull one perpendicularly from the other, in which case the free water surface round the edges will take a (cylindrical) curvature of radius $\frac{1}{400}$th of an inch, would be the weight of a six-inch square prism of water about 5 inches high, *i.e.* between 6 and 7 pounds' weight. If the film were of half that thickness (at the edges) the force required would be double. Thus, as J. Thomson has pointed out, two flat slabs of ice, hanging side by side on a horizontal wire, with a film of water between them, are pressed together with a force which may much exceed the weight of either: and may therefore freeze together even in a warm room. When a mere drop of water is placed between two very true glass planes the relief of pressure produced enables the atmospheric pressure to force them closer together, and this effect increases, not only by the enlargement of the wetted surfaces, but by the increase of curvature round the edges of the flattened drop. The pressure producible in this way is very great, and may crack large sheets of plate glass (if there be portions not very true) when they are laid on one another with a drop of water between them. On the other hand, a few small drops of mercury, interposed here and there between the plates, form an exceedingly perfect elastic cushion.

288. There are many common phenomena whose explanation is easily traced to the action of capillary forces. Thus air-bubbles, sticks, and straws floating on still water, appear to attract one another; and gather

into groups, or run to the edge of the containing vessel. This is always the case with any two bodies, each of which is wetted by the water, and it is also true when neither is wetted. But when one of the bodies is wetted, and the other is not, they behave as if they repelled one another. The explanation is easily given :—either from the point of view of the various forces called into play by the displacement of the water, or (more simply) by the consideration of the whole energy of the liquid as depending on the relative position of the floating bodies (§ 263) and the consequent displacement of the surface.

A needle, or even a (very small) pellet of mercury, may easily be made to float on water. The hydrostatic condition requires merely a depression of the surface, so that the water displaced may be equal in weight to the floating body; but, that this displacement may take place, the angle of contact must be made greater than 90°, which is at once ensured if the needle be very slightly greased. Thus we explain how water-flies run on the surface of a pool.

In the same way we can explain why a piece of wood is not wetted when it is dipped into water whose surface is covered with lycopodium seed; and why mercury can be poured in considerable quantity into a bag of gauze or cambric without escaping through the meshes. (§ 100.)

An air-bubble in water assumes a spherical form, even when it is in contact with the side of a glass vessel, and a very small globule of mercury laid on glass becomes almost spherical. But an air-bubble on the side of a glass vessel containing mercury is flattened out, while a drop of water on clean glass spreads itself out indefinitely. In all these cases the angle of contact at once explains the result.

The difficulty of obtaining a *clean* surface of water or mercury depends upon the great surface-tension of these liquids relatively to that of the majority of other substances. From the reasoning of § 281, and the data of § 285, we see that water ought to spread indefinitely over a clean surface of mercury.

289. The form of section of a (cylindrical) liquid surface, in contact with a plane solid surface, is easily deduced from the hydrostatic principle that the elevation (or depression) at any point is proportional to the relief (or increase) of pressure, *i.e.* to the one curvature. Hence it must be the curve of flexure (§ 237) of a very long uniform elastic wire, with a kink in it, under the action of tensions at its ends; for at every point of that curve the curvature is proportional to the distance from the line in which the stress acts. Hence we can at once find the form in which the liquid surface meets a plane solid face, whether it be vertical or not, by drawing the corresponding elastic curve and taking account of the inclination of the plate and of the angle of contact. When the liquid surface is between two glass plates, inclined at any angles to the vertical, but having their line of intersection horizontal, the form of the cylindrical surface is given by one of the more complex forms of the elastic curve.

290. The surface-tension of liquids diminishes with rise of temperature. And Andrews showed that, as liquid carbonic acid is gradually raised to its critical temperature, the curvature of its surface in a capillary tube gradually diminishes.

291. Lord Kelvin[1] showed that there is a definite vapour-pressure, for each amount of curvature of a liquid

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[1] *Proc. R.S.E.*, 1870.

surface, necessary to equilibrium. It is less as the surface is more concave, greater as it is less concave or more convex. Hence precipitation of water-vapour will, *ceteris paribus*, take place more rapidly the more concave (or the less convex) is the surface of that already deposited. Thus, as Clerk-Maxwell pointed out, the larger drops in a cloud must grow at the expense of the smaller ones. The explanation of these curious facts is given by the kinetic theory much in the same way as is that of the effect of the curvature of the discs of a *Radiometer*.

So great a pressure of vapour would be necessary for the existence of *very* small globules of water (in the nascent state of cloud, as it were), that, as Aitken has shown, condensation cannot commence in free air without the presence of dust-nuclei. The more numerous these are, the smaller is the share of each, and thus we have various kinds of fog, mist, and cloud.

292. Many extremely curious phenomena, due in great part to surface-tension, have been investigated by various experimenters, especially Tomlinson. Thus different kinds of oils can be distinguished from one another, or the purity of a specimen of a particular oil may be ascertained, by the form which a drop takes when let fall on a large, clean, water surface. In some cases a drop of oil does not spread entirely over a liquid surface, but forms a sort of lens. The angles at which its faces meet one another, and the surface of the liquid, are then to be determined from the respective surface-tensions by the triangle of forces, as in § 281.

Again, when a drop of an aqueous solution of a salt, say permanganate of potash or some other highly-coloured substance, is allowed slowly to descend in water, it at first takes the form of a vortex-ring, bounded, of course, by a film of definite surface-tension. But, as diffusion proceeds, it would appear that this film becomes weaker at certain places (just as in the case of wine, § 269), and consequently unstable. Be this as it may, the ring breaks into segments, each of which is (as it were a new drop, which behaves as the original drop did, though somewhat less vigorously. Thus we have a very curious appearance, almost resembling the development of a polyp; the number of distinct individuals being markedly greater in each successive generation. With a drop of ink these developments take place so fast that the eye can scarcely follow them.

The phenomena of surface-tension were found by Bosscha to be exhibited, in some forms, by smoke. And Van der Waals showed that smoke stands lower in the moistened branch of a U–tube than in the dry one, exhibiting a convex surface like that of mercury.

293. We may now say a word or two as to the extreme limits at which the molecular forces are sensible. It is not at all remarkable that the various estimates differ widely from one another, for they are all obtained by processes more or less indirect. They all agree, however, in giving small values. Experiments of Plateau on soap-films, and of Quincke on the behaviour of water and thinly-silvered glass, give only about 1/500,000 of an inch. It is probable that the limits vary somewhat with the nature of the substance experimented on; and the question is certainly connected, in no remote manner, with the differences in the *critical temperatures* (§ 194) of various substances.

294. The separation into drops, of a liquid column slowly escaping into air from a small hole in the bottom of a vessel, can be studied by examining it by the light

of electric sparks rapidly succeeding one another. It is a phenomenon similar to that which we have described in § 275, when a cylindrical film is drawn out between two funnels. When the liquid is a very viscous one, as treacle or Canada balsam, especially if its surface-tension be small, the viscosity greatly retards the development of this effect of instability; and such liquids can, like melted glass, be drawn into fine continuous threads. This property sometimes gets the special name of *Viscidity*.

295. The propagation of ripples, as Lord Kelvin showed,[1] is also due mainly to surface-tension. The experimental proof is given by the fact that the shorter are the ripples the faster they run, while ordinary oscillatory waves in deep water, propagated by gravitation, run faster the longer they are.

[This affords a good example of the application of what is called the principle of *Dynamical Similarity*; *i.e.* the effect of *scale* upon physical phenomena. It is, of course, merely a question of *Dimensions* as in § 64. Various instances of the application of this principle have already been given, *e.g.* §§ 40, 167, 228, 284, etc.]

In two similar ripples, of different wave-lengths, the forces are independent of the lengths, the ranges are directly as the lengths, and the masses of water are as the squares of the lengths, of the ripples. Hence the rates of propagation are inversely as the square root of the wave-length. In similar oscillatory gravitation waves, on the contrary, the forces are as the squares of the lengths, the ranges as the lengths, and the masses as the squares of the lengths, and the rate is directly proportional to the square root of the wave-length.

Thus very short ripples run almost entirely by surface-tension, while long ripples and short waves run partly by gravity partly by surface-tension. Lord Kelvin has shown that the *limit* between waves and ripples in water, which is the slowest-moving surface disturbance, has about 2/3 inch as its wave-length, and runs at a speed of 9 inches per second. Every shorter disturbance runs mainly by surface-tension, and may be called a ripple; every longer one runs mainly by gravitation, and may be called a wave. Fairly accurate determinations of surface-tension have been obtained by measurement of the lengths of ripples produced when a vibrating tuning-fork (of known pitch) rests against a trough containing a liquid.[1]

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[1] *Phil. Mag.*, 1871, ii. p. 375.

296. When a solid is exposed to a gas or vapour, a film is deposited on its surface which, in many cases, introduces confusion in weighings, etc. Thus, if a dry platinum capsule be carefully weighed, then heated to redness and weighed again immediately after it has cooled, it is found to be lighter. If left exposed to the air it gradually recovers its former weight. In so far as this effect is a purely surface one, it is increased in proportion as the surface of a given mass of the solid is increased. Thus "spongy" platinum, as it is called—*i.e.* platinum in a state of very minute division (obtained by reducing it by heat from some of its salts)—exhibits the phenomenon to a notable extent. Dobereiner showed that a jet of hydrogen can be set on fire, by the heat developed when it is blown against spongy platinum which has been exposed to the air. The platinum is heated to redness by the combination of the oxygen film, already condensed on its surface, with the hydrogen which suffers condensation in its turn.

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[1] C. M. Smith, *Proc. R.S.E.*, 1890.

Another remarkable form of experiment, analogous to this, consists in heating a helix of platinum wire to incandescence in the flame of a Bunsen lamp, turning off and then immediately turning on again the supply of gas; for the wire remains permanently red-hot in the explosive mixture of air and coal-gas; without, however, reaching a high enough temperature to inflame it again.

The amount of surface really exposed by finely porous bodies, especially (as Hunter[1] showed) cocoa-nut charcoal, is enormous in comparison with their apparent surface; and in consequence they are able to absorb (as it is called) quantities of gas altogether disproportionate to their volume. Even ordinary charcoal, when heated red-hot (to drive out the gases already condensed in its pores) and allowed to cool in an atmosphere of carbonic acid gas, absorbs from sixty to eighty times its volume of the gas. If it be then introduced into a tube full of mercury it can be made, by heating, to disgorge this gas, which it reabsorbs as it cools. This property has been utilized for the production of very high vacua; as much as possible of the gas being removed by an air-pump while the charcoal is hot, and the greater part of the remainder being absorbed when it cools.[2]

The student may easily understand the immense addition to the surface of a body, which is caused either by pores or by fine division, if he reflect that a cube, when sliced once parallel to each of its pairs of faces, obviously has its whole surface doubled.

297. There is another form of action, analogous to this, produced by certain substances, such as peroxide of

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[1] *Chem. Soc. Journal.*, 1865–72.

[2] Dewar and Tait, *Proc. R.S.E.*, 1874.

manganese, when in a state of fine division. When a stream of oxygen, containing ozone, is passed through the powder it emerges as oxygen alone. The ozone has been reduced to the form of oxygen by what is called *Catalysis;* the oxide of manganese is practically unaltered.

298. What is called the *solution* of a gas in a liquid is, in many respects, analogous to the condensation on the surface (or in the pores) of a solid.

The empirical laws of this subject, originally given by Henry and by Dalton, have been verified for moderate ranges of pressure by Bunsen.

According to Henry, when a solution of a gas is in equilibrium with the gas itself, the amount dissolved in unit volume of the liquid is proportional to the pressure of the gas. The coefficient of proportionality diminishes rapidly with rise of temperature.

To this Dalton added that each constituent of a gaseous mixture is dissolved exactly as it would have been had the others not been present.

It appears that the heat disengaged in solution is always greater than that due to the mere liquefaction of the gas. Hence the phenomenon is, to a considerable extent, of a chemical character; and thus we are prepared to find great differences in the absorption of the same gas by different liquids. Thus carbonic acid is 2·5 times more soluble in alcohol than in water; while it is 1·8 times more soluble in water at 0° C. than in water at 15° C.

CHAPTER XIII.

DIFFUSION, OSMOSE, TRANSPIRATION, VISCOSITY, ETC.

299. Though we cannot mark a special group of the particles of any one liquid or gas, so as to enable us to *see* how they gradually mix themselves with the others, we have almost perfect assurance that they do so. This assurance is based partly upon the relative behaviour of two miscible liquids, or two gases, put in contact with one another; partly upon the results of the kinetic theory, which have been found fully to explain at least the greater number of the phenomena ordinarily exhibited by gases. Thus, altogether independent of the convection currents due to differences of temperature, there goes on, in every homogeneous liquid or gas, a constant transference of each individual particle from place to place throughout the mass. In homogeneous solids, at least, it seems probable that there is no such transference, but that each particle has a mean, or average, position relatively to its immediate neighbours, from which it suffers only exceedingly small displacements.

300. True diffusion, which is much more rapid in gases than in liquids, is essentially a very slow process compared with those convection processes which are mainly instrumental in securing the thorough intermixture of the various constituents of the air or of

dissolved salts with the ocean water. For its careful study, therefore, great precautions are required, with a view to the preservation of uniformity of temperature, as the only mode of preventing convection currents. We will suppose that these precautions have been taken.

If, by means of a tube (fitted with a stop-cock) which is adjusted at the bottom of a tall glass cylinder nearly full of water, we cautiously introduce by gravity a strong solution of some highly-coloured salt (such as bichromate of potash), the solution, being denser than the water, forms a layer at the bottom of the vessel. If we watch it from day to day we find that, in spite of gravity, the salt gradually rises into the water column, which now shows an apparently perfectly continuous gradation of tint from the still undiluted part of the solution up to the as yet uncontaminated water above. The result irresistibly suggests an analogy with the state of temperature of a bar of metal which is exposed to a source of heat at one end. The analogy would be almost complete if we could prevent loss of heat by the sides of the bar; for experiment has shown that, just as the flux of heat is from warmer to colder parts, and (*ceteris paribus*) proportional to the gradient of temperature, so the diffusion of the salt takes place from more to less concentrated solution, and at a rate at least approximately proportional to the gradient of concentration. This is, possibly, not quite the case at first, when there are exceedingly steep gradients of concentration, for then (see § 292) there is probably something akin to a surface-film which for a time behaves somewhat like that between two liquids which do not mix. This is forcibly suggested by the result of rough stirring of the contents of a vessel with parallel glass sides, in which there is a layer of strong brine with clear water above it; especially if a horizontal beam of sunlight, from a distant aperture in the shutter of a dark room, be made to pass through the vessel, and be received on a sheet of paper placed a few inches behind. However rough the stirring, if it be not too long continued, the mixture is soon seen to settle into layers of different densities; and time is required before diffusion does away with the steep gradients of concentration which have been produced between the layers. These effects can be produced again and again in the same mixture, and show how very much more rapid is the mixing when aided by rough mechanical processes than when left entirely to the slow but sure effects of diffusion. The effect of the stirring is to produce immensely extended surfaces of steep gradient of concentration all through the mixture, and thus greatly to accelerate the natural action of diffusion, to which the final result of uniform concentration is really due.

301. The first accurate experiments on this subject are due to Graham,[1] who employed various very simple but effective processes. He showed that while the rate of diffusion varied considerably with the substances employed, these could be ranged in two great classes, *Colloids* and *Crystalloids*, the members of the first class having very small diffusivity compared with those of the second. Thus he found that the times employed for equal amounts of diffusion in water were relatively as follows:—

Hydrochloric Acid	1
Common Salt	2·33
Sugar	7
Albumen	49
Caramel	98

[1] *Chemical and Physical Researches*, collected and reprinted, 1876.

He also verified that the rate of diffusion of any one substance is proportional to the gradient of concentration, and added the important fact that rise of temperature has a marked effect in accelerating the process.

302. The subject has since been elaborately investigated by various experimenters, and absolute values of diffusivity have been calculated from their experiments as well as from those of Graham.

Following the analogy with heat-conduction, we may define, after Fourier's method, as follows:—

The diffusivity of one substance in another is the number of units of the substance which pass in unit of time through unit of surface, when the gradient of concentration perpendicular to the surface is unit of substance per unit of volume per unit of length.

If we use the C. G. S. system, in which unit of length is a centimètre, unit of mass a gramme, and unit of time a second, the numbers obtained would be exceedingly small, so that the C. G. S. system is departed from in practice to the extent of making a day the unit of time. With this we have, according to Stefan's calculations from Graham's results:—

	Temperature C.	
Hydrochloric Acid	5°	1·74
Common Salt	5°	0·76
,, ,, ...	10°	0·91
Sugar	9°	0·31
Albumen	13°	0·06
Caramel	10°	0·05

The meaning of this is that, for instance at 10° C., in water which so holds common salt in solution that there is one gramme per cubic centimètre more in each horizontal stratum, than in the stratum one centimètre above it, the upward progress of the salt is at the rate of 0·91 gramme through each square centimètre per day. [Solutions of common salt differing by whole grammes of salt per cubic centimètre are, of course, only a pleasant fiction of the C. G. S. system.]

303. Fick, Voigt, Hoppe-Seyler, H. Weber, and many others, have greatly extended Graham's work; some using his process (with slight variations), others employing processes depending upon special physical results (such as rotatory polarisation, or electromotive force) due to the salt which is diffusing. It is probable that very good measures may be obtained, though the method would be laborious, by using a narrow tank with parallel glass sides (as in § 300), and observing, from time to time, the greatest refraction suffered by any part of a horizontal beam of sunlight transmitted through the heterogeneous liquid, the tank having been originally half filled (as in § 300) with a strong solution of a salt, under pure water.[1] Lord Kelvin introduced a rough-and-ready method by letting down into the diffusion column a number of glass beads, containing more or less of air, and therefore having, each as a whole, different mean densities, and observing from day to day the position of the stratum in which each floated in equilibrium. This method would probably be the best of all, could we only make the beads small enough, so as not to trench upon too many strata at once, and could we also make certain that neither air-bubbles nor crystals should develop upon them. The latter condition, however, is practically unattainable.

304. It seems that the idea of comparing diffusion with heat-conduction was originally propounded by Berthollet

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[1] Tait, "On Mirage," *Trans. R.S.E.*, 1881.

before Fourier published his great investigations on the latter subject; but Fick was the first to revive and develop it in more recent times.

The physical explanation of the cause of diffusion of liquids in one another, or of solids in a liquid, is vastly more complex and difficult than that of the diffusion of gases, though, in some of their coarser features, the first two of these are closely analogous to the last. In the words of Clerk-Maxwell: "It is easy to see that if there is any irregular displacement among the molecules of a mixed liquid, it must, on the whole, tend to cause each component to pass from places where it forms a large proportion of the mixture to places where it is less abundant. It is also manifest that any relative motion of two constituents of the mixture will be opposed by a resistance arising from the encounters between the molecules of these components. The value of this resistance, however, depends, in liquids, on more complicated conditions than in gases, and for the present we must regard it as a function of all the properties of the mixture at the given place—that is to say, its temperature and pressure, and the proportions of the different components of the mixture."

305. The interdiffusion of gases is thus, theoretically, a simpler question than that of liquids; and has been developed, from the basis of the kinetic theory of gases, into an almost complete explanation of the observed phenomena. We cannot here introduce the mathematical part of the investigation, as it involves analysis of a kind foreign to the range of an elementary book; but we simply state that the equations ultimately arrived at are, in their simplest form, closely analogous to those obtained by Fourier for heat-conduction in a homogeneous isotropic

solid. This part of the theory we owe mainly to Clerk-Maxwell. The experimental part has been well supplied by Loschmidt.[1] The following numbers give an idea of his values of interdiffusivity of pairs of gases, in a mixture at a pressure of one atmosphere. We have preserved only two significant figures, though the measures (which are in C. G. S. units) were given to four.

Carbonic Acid and Air	0·14
Oxygen and Hydrogen	0·72
Carbonic Acid and Hydrogen	0·55
Carbonic Acid and Carbonic Oxide	0·14
Carbonic Oxide and Hydrogen	0·64

According to the theory, as given by Maxwell, these quantities should be nearly in inverse proportion to the geometrical mean of the densities (at one atmosphere) of the two gases. The higher parts of the theory of this subject are very complex and difficult, and cannot yet be considered as at all satisfactorily developed.

306. Returning to the consideration of liquids, we meet with certain very curious phenomena when two miscible liquids are separated by a membrane such as a diaphragm or septum of bladder, or of parchment paper. These are usually arranged under the general title of *Osmose*, sometimes pedantically divided into endosmose and exosmose. The first careful examination of them we owe to Dutrochet, but the earliest observation recorded is due to Nollet in the first half of last century.

The main phenomenon, of which all the others are more or less complicated varieties, is simply that different liquids pass at different rates through a porous membrane.

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[1] *Sitzungs-Berichte d. Kais. Ac. zu Wien*, 1870.

Thus Nollet immersed in water a vessel full of alcohol, tightly closed by a piece of bladder, and was surprised to find that the contents soon increased to such an extent as almost to burst the bladder. He then filled the vessel with water, tied on the bladder, and immersed the whole in alcohol, when the reverse effect was obtained; the contents of the vessel diminished and the bladder was forced inwards. Strange to say, after so good a commencement, he contented himself with recording the two observations.

307. The phenomenon is so obviously connected with many processes which go on in living bodies, whether vegetable or animal, that it has attracted the attention of physiologists as well as of physicists, and an immense mass of observations on various forms of it has already been accumulated.

Its theoretical explanation is much more complex than that of ordinary liquid diffusion, because it is found that the *material* of the septum plays an important, often a paramount, part in determining the rate, and sometimes even the direction, of the osmose in a trial with two given liquids.

Osmose is undoubtedly a case of ordinary diffusion, complicated by the molecular actions between the material of the septum and the various liquids employed. Thus there need be no more reason for surprise that a liquid, such as the sap in plants, should be osmotically raised to great heights against gravity, than that water should rise in a capillary tube, or that bichromate of potash should (§ 300) diffuse upwards in a column of water.

308. Something very similar to osmose can be obtained by ordinary diffusion, when horizontal strata of two liquids are separated by a stratum of a third liquid of

intermediate density. Sometimes one or other of the extremes alone passes through the intermediate layer, sometimes both diffuse into it. A beautiful method of gradually developing chemical actions which, on the large scale, would produce dangerous explosions, is thus suggested. When nitric acid, water, and alcohol are the three liquids, the chemical action takes place slowly where the two extreme liquids meet, as they diffuse towards one another through the water-septum.

309. Though the theory is but imperfectly understood, the practical applications of osmose have been developed to an important extent. Of these we need here mention only the process of *Dialysis*, due to Graham. The distinction between Colloids and Crystalloids, in their behaviour as regards a porous septum, is even more marked than in direct liquid diffusion. Hence, when a mixture of colloids and crystalloids, in solution, is placed on one side of a bladder or a piece of parchment paper, and pure water on the other side, it is practically the crystalloids alone which pass through the septum into the water. If the colloids be originally in enormous excess, one repetition of the process on the mixture which has passed through the septum is sufficient to separate the crystalloids almost entirely from the colloids.

This process is of very great importance as an auxiliary to chemical analysis in medico-legal questions:—for the more common of the violent poisons are with few exceptions crystalloids, and can be easily and almost completely separated, by dialysis, from the large admixture of colloids in which they are usually found in the viscera.

310. Graham, in his extensive series of experiments on the passage of gases through various solids with holes

or pores, recognised several quite distinct processes, each with its own laws.

When a gas is maintained at constant pressure on one side of a very thin non-porous plate, which has a small hole in it, there being vacuum at the other side, the process of passage is called *Effusion*. This may be looked on as at least roughly analogous to the passage of a liquid through the orifice. The closer consideration of it belongs to *Thermodynamics*. The work done on unit volume as it passes out is directly as the pressure, the kinetic energy acquired is measured by the density and the square of the speed of effusion conjointly. Hence, under the same pressure, the speed of effusion is inversely as the square root of the density. This result was very nearly realised in Graham's experiments; witness the following:—

	Time of Effusion.	Theoretical Time.
Air	1·0	1·0
Nitrogen	0·984	0·986
Oxygen	1·050	1·051
Hydrogen	0·276	0·263
Carbonic Acid . . .	1·197	1·236

The only discrepancies which call for notice are with hydrogen and carbonic acid. But Graham was able to show from the results of another of his series of experiments, that these discrepancies are due to the fact that the perforated plate was not infinitely thin, and that the aperture therefore behaved like a very short capillary *tube*. This explanation is fully borne out by the fact that the discrepancies are *in opposite directions* for these two gases, and that this characteristic difference is *required* by the mode of explanation.

From these experiments Graham concluded that the law of this process is analogous to that of diffusion without a septum. Bunsen has applied the result to the construction of a very excellent instrument for measuring the density of a gas.

311. *Transpiration* is the name given by Graham to the passage of a gas under pressure through a capillary tube. The results obtained were of a much more complex character than in the case of effusion; and the law of the process, so far as it could be ascertained by experiment alone, was of a different form. Capillary tubes, varying in length from 20 feet down to a few inches, were employed. It was found that the material of the tube had no influence; hence it has been suggested that the tube becomes lined with a film of the gas, and that the key to the difficulties of the problem is to be sought mainly in connection with viscosity. The rate of transpiration of hydrogen is only double of that of nitrogen, while that of carbonic acid is much greater than that of oxygen:—

	Limiting Transpiration Times in very fine Capillaries.
Oxygen	1·000
Air	0·901
Nitrogen and Carbonic Oxide	0·875
Hydrogen	0·437
Carbonic Acid	0·727

The two last results show the foundation of the explanatory remark towards the end of last section.

The following are some of Graham's comments on this very curious subject:—

"The times of oxygen, nitrogen, carbonic oxide, and air, are directly as their densities, or equal *weights* of these gases pass in equal times. Hydrogen passes in half the time of nitrogen, or twice as rapidly for equal volumes. The result for carbonic acid appears at first anomalous. It is that the transpiration time of this gas is inversely proportional to its density, when compared with oxygen. It is to be remembered, however, that carbonic acid is a compound gas, containing an equal volume of oxygen. The second constituent carbon which increases the weight of the gas, appears to give additional velocity to the oxygen in the same manner and to the same extent as increased density from pressure, or from cold (as I believe I shall be able to show), increases the transpiration velocity of pure oxygen itself. A result of this kind shows at once the important bearing of gaseous transpirability, and that it emulates a place in science with the doctrines of gaseous densities and combining volumes.

"The circumstance that the transpiration time of hydrogen is one-half of that of nitrogen, indicates that the relations of transpirability are even more simple in their expression than the relations of density among gases. In support of the same assertion may be adduced the additional fact; that binoxide of nitrogen, although differing in density, appears to have the same transpiration time as nitrogen. Protoxide of nitrogen and carbonic acid have one transpiration time, so have nitrogen and carbonic oxide, as each pair has a common density."

312. When one gas is separated from another, or from a vacuum, by a septum of compressed graphite (§ 53), the law, and even the *rate*, of passage come to be very nearly the same as those of ordinary gaseous diffusion. Thus gases pass through such a septum at rates inversely as the square roots of their densities, as in effusion. If the septum is made of plaster of Paris, the results become partially complicated by transpiration. This source of

confusion is practically non-existent when the septum is made of "biscuit-ware," as it is technically called; and the same may be said of all the finer kinds of unglazed earthenware. Here the pores are so fine that, as Graham says, the action ceases to be molar and becomes molecular. Each particle acts, as it were, on its own account. Hence, when a mixture of two gases of different densities is placed on one side of such a septum, the less dense gas passes in greater percentage than the denser, and we have *Atmolysis*:—a mode of separating different gases somewhat akin to dialysis (§ 309). There are few physical experiments more striking and suggestive than the simple one of surrounding, with an atmosphere of coal gas, the bulb (made of unglazed clay) of an arrangement like a large ordinary air-thermometer. The rapidity with which the gases pass through the bulb is extraordinary.

313. But when the septum is made of caoutchouc the process of penetration is quite different. The septum now acts as a colloidal body, not as a porous one; and the gas combines in an imperfect *chemical* manner with the matter of the septum, in which it diffuses (in the ordinary sense of the term), until it reaches the other side and is set free. Thus the small toy-balloons of thin india-rubber, when originally filled with hydrogen, soon collapse. On the other hand, when they are blown with air and then immersed in an atmosphere of hydrogen, they rapidly swell and burst.

The same phenomenon is beautifully shown by blowing a soap-bubble with carbonic acid gas. For the gas dissolves in the liquid film, diffuses through it, and escapes into the air, so that the bubble soon collapses. Similarly, an ordinary soap-bubble made to float on carbonic acid gas expands, gradually sinks, and finally bursts.

A good instance of gaseous diffusion is afforded by evaporation of water, or other liquid, at temperatures below the boiling point, when air is present. For the process goes on until the vapour in contact with the liquid has a pressure determined solely by the temperature, and by the curvature of the liquid surface. When a layer of vapour of the proper pressure has once formed at the surface, the resistance to its diffusion is so considerable that, unless there be wind or convection-currents, the rate of evaporation is reduced to that of diffusion; and vapour is formed (at the liquid surface) only as fast as that which is already formed is able to get away. By weighing, from time to time, a test-tube of known length, which has a layer of liquid at the bottom and is open at the top, fair measures of the rate of diffusion in air, of vapours heavier than air, can be obtained.

314. Very curious results have been obtained by Deville and Troost with reference to the rapid passage of various gases through heated cast-iron. Carbonic oxide is one of these, and as this is a highly poisonous gas, the matter is one of great importance in relation to the heating of rooms by stoves. They also showed that highly heated platinum is freely pervious to hydrogen. Graham's researches on the behaviour of palladium with respect to hydrogen have afforded the means of obtaining similar effects even at temperatures far below red-heat; and, quite recently, v. Helmholtz and Root have proved that platinum is pervious to hydrogen even at ordinary temperatures. Thus the question is one of importance, not alone from the sanitary point of view, nor from the point of view of its purely scientific explanation, but also from the very important point of view of the construction of gas-thermometers for the measurement of

high temperatures, in which the recipient must necessarily be made of some practically infusible metal. The whole of this part of the subject, however, has a specially chemical interest, so that we are not called on to discuss it further.

315. We have already employed the word *Viscosity* in two somewhat different applications. In our general discussion of common terms (§ 37) we spoke of it as applied to liquids, and also, by parity of results, to gases. But in § 249 we used it as denoting a property possessed even by the most elastic of solids.

We must now consider, more carefully, its application to fluid motion.

And, first, as regards liquids. Questions such as were briefly touched upon in § 37 belong, in their full development, where eddies present almost insuperable difficulties, to *Hydrokinetics*, and are therefore not to be treated farther in this work. But the passage of a liquid, under pressure, through a capillary tube, is (so far as it is amenable to elementary mathematical treatment) part of our subject. So is the torsional vibration of a disc, in its own plane, when it is suspended by a wire and immersed in a fluid, especially when, as in Clerk-Maxwell's experiments on gases, other two discs, are *fixed* near and parallel to it, on opposite sides. So far as liquids are concerned, these forms of experiment were carefully worked out by Poiseuille and by Coulomb respectively, and have since been extended, with various modifications, by v. Helmholtz, O. E. Meyer, etc. Later, Graham (as we have just seen), Clerk-Maxwell, and many others, have applied one or other of these forms of experiment, more or less modified, to the determination of the viscosity of gaseous bodies.

316. Before going farther, we must define precisely what we mean by viscosity; and the definition will, of course, show how it is to be measured.

Suppose a layer of fluid, of unit thickness, to fill the interval between two plane surfaces, of indefinite extent, to which the fluid adheres. When one of these surfaces is made to move in a given direction parallel to the other, with unit speed, the tangential force on either per unit of surface is the measure of the viscosity.

Hence, if, in Fig. 1, § 38, v be the speed at depth y, the tangential stress per unit surface of the layer at that depth is—

$$\kappa\frac{dv}{dy}$$

where κ is the viscosity. The dimensions of Viscosity, therefore, differ from those of Rigidity (§ 178) simply by the time unit; *i.e.* as the dimensions of velocity differ from those of acceleration. This may be seen at a glance from the equation of § 250.

The establishment of a simple working definition, such as that above, leads at once to the formation of the proper equations of motion in all problems of this kind. The process is precisely the same as that adopted by Fourier in his definition of Heat Conductivity; and it is curious to see how all who have, in modern times, treated viscosity without using Fourier's method, have fallen into the vague and misleading methods of Fourier's predecessors.

317. If, with this definition, we investigate the motion of a liquid in a capillary tube, when it has become steady, we are led to the result (fully borne out by the experiments of Poiseuille[1]) that, *ceteris paribus*, the discharge

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[1] *Mém. des Savans Étrangers*, IX., 1846.

in a given time is proportional to the fourth power of the radius of the bore. (Compare § 228.)

For the solution of the problem we assume that the motion is everywhere parallel to the axis of the tube, and with speed dependent only on the distance from it. Let κ be the viscosity, and consider the tangential stress (per unit length of the tube) on the surface of a cylindrical layer of liquid of radius r, concentric with the tube. If v be the speed of that layer, the amount of the stress is (by the definition above)

$$2\pi r\kappa\frac{dv}{dr}.$$

Hence the difference of the tangential forces on the surfaces of a cylinder of liquid of thickness δr is

$$2\pi\kappa\frac{d}{dr}\left(r\frac{dv}{dr}\right)\delta r.$$

But, as the motion is not accelerated, this must be equal and opposite to the difference of the pressures on the ends of the cylinder, which is (per unit length)

$$-2\pi r\delta r\,.\frac{dp}{dx},$$

where x is measured parallel to the axis. This must obviously be independent of x, and, as the motion is always very slow under the conditions, p is approximately independent of r. Hence

$$\frac{dp}{dx}=-a,$$

a constant, whose obvious value is the difference of pressures at the ends, divided by the length, of the tube.

Thus we have the very simple equation

$$-ar=\kappa\frac{d}{dr}\left(r\frac{dv}{dr}\right).$$

This gives

$$r\frac{dv}{dr}=-\frac{a}{2\kappa}r^2,$$

the integration constant being zero, because otherwise we should have finite tangential stress on an infinitely small filament along the axis.

Thus, if r_0 be the radius of the bore of the tube, and if we assume that v is nil when $r=r_0$, (*i.e.* that there is no finite slipping of the liquid along the walls of the tube)

$$v=\frac{a}{4\kappa}(r_0^2-r^2).$$

The whole volume of liquid which passes in unit of time through each cross section is

$$2\pi\int_0^{r_0} rv\,dr=\frac{\pi a}{2\kappa}\int_0^{r_0}(r_0^2-r^2)r\,dr=\frac{\pi a}{8\kappa}r_0^4.$$

318. This expression enables us at once to calculate the values of κ from experiments such as those of Poiseuille Their agreement with the formula above is very close throughout, the bores, lengths, and pressures being varied within wide limits. The most remarkable additional feature which Poiseuille recognised is the rapid diminution of viscosity of water with rise of temperature. In C. G. S. units his experiments give, approximately,

Temperature C.	Coefft. of Viscosity.
0°	0·018
10°	0·013
20°	0·010

This means that there is a tangential stress of 0·018 dynes per square centimètre on each of two parallel planes, one centimètre apart, when one is moving relatively to the other at the rate of one centimètre per second, and when the interspace is filled with water at 0° C.

It is well to note that from 0° to 10° C. the viscosity of water falls off at the rate of 2·8 per cent. per degree. Compare this with the corresponding increase of the rate of diffusion of common salt in water (§ 302), which, by Graham's results, is about 4 per cent per degree of temperature.

319. The oscillation method of Coulomb is troublesome in the complex mathematical details to which it leads, and even more so in the experimental precautions which it requires. It has been carefully worked out, from both points of view, by many different physicists, including especially O. E. Meyer. Modifications of it have been employed by v. Helmholtz and others. The theoretical part of the investigation, which is very complex, was first developed by Stokes, who applied it to the reduction of pendulum experiments.

The results of Meyer are about a sixth greater than those of Poiseuille. Those of v. Helmholtz and v. Pietrowski,[1] in which liquids were contained in an oscillating sphere, were complicated by finite slipping, which led to a new problem. Their result for water is about one-fourth greater than that of Poiseuille.

According to Schöttner,[2] the coefficient of viscosity of glycerine (in C. G. S. units) sinks, from about 42 at 3° C., to little more than 8 at 20° C.

320. The Viscosity of a gas can be calculated, at least approximately, from the Kinetic theory, for it is easy to see that it must depend upon the transference of momentum

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[1] *Sitzungsber. der K. Ac. in Wien*, 1860.

[2] *Ibid.*, 1878, p. 686.

by the interchange of particles between two contiguous layers of the gas which have relative velocity.

Clerk-Maxwell, who first gave the theory of this subject, found that the viscosity is independent of the density in each particular gas, and *increases* with rise of temperature, being directly proportional to the square root of the absolute temperature.

But his experiments on air, made (§ 315) by the oscillation method, gave (in C. G. S. units) the formula

$$0{\cdot}000{,}000{,}685(274+t)$$

where t is the temperature centigrade. Here a different temperature-law appears.[1]

Maxwell also showed that in oxygen the viscosity is greater than in air, and in carbonic acid less. In hydrogen it is about half as great as in air. Theoretically it is as the density of the gas, and the mean free path of a particle, conjointly. The mean free path depends upon the *size* of the particles, being (*ceteris paribus*) inversely as the squares of their diameters. [Compare with Graham's results above, §§ 310, 311.] This subject has since been elaborately investigated by Meyer, Kundt and Warburg, and many others, but the exact law of the temperature-variation is still uncertain.

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[1] Stokes (*Phil. Trans.*, 1886) attributes this discrepancy to non-parallelism of the fixed and oscillating plates.

CHAPTER XIV.

AGGREGATION OF PARTICLES.

321. This chapter must be a very short one, because, though experimental *facts* are to be had in profusion, the subject, as a whole, has not yet been raised to the higher level of *Science* from that of the mere preliminary "beetle-hunting or crab-catching stage." Parts of it are already much further advanced. The geometry of crystalline forms has been very fully developed and systematised. The physical properties of the aggregate have been scientifically developed, as we have seen, mainly from the basis of Hooke's Law for solids and liquids, and of Boyle's Law for gases; and the formulation of these laws has enabled us to discuss, with something like a secure foothold, the deviations from them.

But the mode of formation of the aggregate of particles, at all events in solids and liquids, is a question of much greater difficulty. We still require, in fact, a Kepler to co-ordinate the facts, before there can be a chance for a Newton to group them under some simple but all-embracing statement.

All that our plan permits us to do is to point out briefly how far the Ptolemy and the Copernicus, as well as the Tycho Brahe, of this subject have marshalled the materials for the coming Kepler. The Newton will be later in appearing.

322. In the case of gases a real step to explanation has been taken, but a great part of the very elements of the *Kinetic Theory* (§§ 33, 107) is still obscure and difficult. The earliest suggestion of it is commonly attributed to D. Bernoulli (1738), but the following passage from Hooke's Pamphlet *De Potentia Restitutiva*,[1] shows that essentially the same ideas had been published long before.

"In the next place for fluid bodies, amongst which the greatest instance we have is air, though the same be in some proportion in all other fluid bodies.

"The air then is a body consisting of particles so small as to be almost equal to the particles of the Heterogeneous fluid medium incompassing the earth. It is bounded but on one side, namely, towards the earth, and is indefinitely extended upward being only hindered from flying away that way by its own gravity (the cause of which I shall some other time explain). It consists of the same particles single and separated, of which water and other fluids do, conjoyned and compounded, and being made of particles exceeding small, its motion (to make its balance with the rest of the earthy bodies) is exceeding swift, and its Vibrative Spaces exceeding large, comparative to the Vibrative Spaces of other terrestrial bodies. I suppose that of the Air next the Earth in its natural state may be 8000 times greater than that of Steel, and above a thousand times greater than that of common water, and proportionably I suppose that its motion must be eight thousand times swifter than the former, and above a thousand times swifter than the latter. If therefore a quantity of this body be inclosed by a solid body, and that be so contrived as to compress it into less room, the motion thereof (supposing the heat the same) will continue the same, and consequently the Vibrations and Occursions will be increased in reciprocal proportion, that is, if it be Condensed into half the space the Vibrations and Occursions will be double in number: If into a quarter the Vibrations and Occursions will be quadruple, etc.

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[1] See footnote to § 221. Also the quotation from Boyle (§ 188) where he speaks of the space the air "*possess'd* not *fill'd*."

"Again, if the containing Vessel be so contrived as to leave it more space, the length of the Vibrations will be proportionably enlarged, and the number of Vibrations and Occursions will be reciprocally diminished, that is, if it be suffered to extend to twice its former dimensions, its Vibrations will be twice as long, and the number of its Vibrations and Occursions will be fewer by half, and consequently its endeavours outward will be also weaker by half.

"These Explanations will serve *mutatis mutandis* for explaining the Spring of any other Body whatsoever."

The modern revival of the theory is due to Herapath; and, a little later, to Joule, who was the first to make definite calculations as to the speed of the particles of a gas necessary for the production of the observed pressure at different temperatures. He stated distinctly that his results were independent of the size of the particles, and of the number of collisions. Krönig is constantly cited, especially in German works, as having advanced the theory; but the only novelty which his paper[1] seems to contain is the somewhat startling, and certainly as yet unverified, result that the *weight* of a gas when in motion is only half what it is when the gas is at rest. [He forgets to take account of the additional impulse due to restitution, § 255.]

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[1] *Pogg. Ann.*, 1856, xcix. p. 319.

The first approach to a thorough treatment of the theory was made by Clausius, who took account of the mutual impacts of the particles, with the consequent conception of the *mean free path;* and who introduced the statistical method of treatment. He was followed by Clerk-Maxwell, and by Boltzmann, and among the three the theory was rapidly developed.

It is already competent, as is shown in works on *Heat* (to which the theory now properly belongs) to explain fully many of the properties of gases; but it still labours under an unsurmounted difficulty, viz. the explanation of the diversity of values of the ratio of the two specific heats (at constant pressure and at constant volume) in various groups of gases. The difficulty is probably due to our ignorance of the interior mechanism of the particles of gases; and it has been greatly enhanced by an apparently unwarranted application of the *Theory of Probabilities*, on which the statistical method is based. But the examination of such questions is foreign to our work.

323. The key to the explanation of the liquid state is undoubtedly to be sought in connection with Andrews' grand discovery of the *Critical Temperature* (§ 194). Whether something akin to this does, or does not, hold with reference to the relation between the solid and liquid states, is a problem which does not appear to have been attacked. It presents, undoubtedly, most formidable difficulties, experimental as well as theoretical, which are heightened by the well-known fact that there are solids whose melting point is *lowered* by pressure. But here, again, we trench on the domain of Heat.

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324. The essential difference between the solid and the liquid states of any kind of matter lies in the fact that *any* distorting stress, however small, if only persistently applied, produces finite change of arrangement among the particles of a liquid, whereas it can in general but infinitesimally alter the relative position of *contiguous* particles of a solid (unless, of course, it be sufficient to produce rupture of the mass). It is barely conceivable that there can be in any case an abrupt transition from one of these states to the other; and, in fact, in the great majority of cases at least, there is known to be a gradual transition from the solid to the liquid states, as the temperature is raised in the vicinity of the melting point, so that there is continuous passage from the state of very plastic solid to that of very viscous liquid. The same thing is observed in various colloidal bodies such as isinglass and other jellies, when made up with different amounts of water. In this connection the student should again read the remarkable statement of Clerk-Maxwell in § 253 above.

Especially on the subject of crystallisation do we appear to get some light from such a view of inter-molecular action. For it would seem that an essential requisite to the formation of a homogeneous crystal must be the comparative freedom of each particle from the influence, direct or not, of all besides those in its *immediate* vicinity. That there is, even in the most homogeneous crystals, still at least a trace of what Maxwell calls the more stable groups, is probably indicated by the existence of *Cleavage Planes*, which are not in general parallel to the more prominent faces of the crystal. In fact, as Sorby has shown, cleavage planes analogous to those of slate-rocks can be developed in the majority of plastic solids by the application of pressure-stress in a direction perpendicular to them. It is thus that slates themselves are formed from a deposit of mud. The proof is obvious from the fact that the planes of cleavage are often inclined at large angles to those of stratification. But we offer these remarks merely as a suggestion, which our limits prevent us from developing.

In this connection it may be well to mention an acute remark of Le Roux[1] to the effect that annealing appears to preserve the state of isotropy characteristic of fusion. He was led to this by remarking that a glass of borate of magnesia, which was ordinarily transparent, became like porcelain or, rather, like white marble when very quickly cooled.

325. Whether all solids tend to acquire ultimately a crystalline structure or not, we can at least seek to find the forms which they are likely to assume in cases (such as that of slow deposition from a state of solution) where each particle is free to choose its position of least potential energy relative to those already deposited. This was long ago very well worked out by Haüy, though (of course) without any reference to potential energy. And the agreement, of the results of such hypothetical calculations, with the observed forms of natural and artificial crystals, shows that we have really made a step in the right direction (though a *very* short one) to the explanation of these singular and beautiful results of the action of molecular forces.

326. The simplest case is that in which the separate particles behave as if they were spherical, *i.e.* as if they exerted equal resultants of molecular and of thermal action (at the same distance) in all directions. Here we

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[1] *Comptes Rendus*, 1867, p. 126.

can call to our assistance the analogy of well-known results as to the piling of shot, etc.

There are two ways in which we may suppose the first plane layer to be laid down; *i.e.* in square order, or in equilateral triangular order. Again there are two ways in which the next layer may be (symmetrically) deposited, *i.e.* particle above particle, or particle above the middle point of each square, or of each equilateral triangle. The two latter cases, however, are really the same arrangement, so far as the relative positions of contiguous particles are concerned; as we see at once by looking at one of the faces of a four-sided pyramid built up on a square base. For in the face the order is triangular. Or, if we remove one edge from the three-sided pyramid built on an equilateral triangular base, we find the particles in the plane of replacement to be in square order.

We find these two forms, apparently so different, in many species both of natural and of artificial crystals. Thus the regular oktahedron, which is merely two four-sided pyramids built on opposite sides of the same square base, and its *hemihedral* form, the regular tetrahedron, which is the three-sided pyramid built on an equilateral triangular base, are both met with in various substances belonging to the *Cubic System*. The measured angles between the several pairs of faces are found to agree exactly with those of the geometrical solids.

327. An imperfectly developed oktahedron, when the imperfection is symmetrical, may assume various forms, all of which are met with in actual crystals. Thus, suppose we remove layer after layer, symmetrically, from the summits of the oktahedron. The new faces thus produced form parts of the faces of a cube; and

we may obtain the cube complete by continuing the process till the last trace of the oktahedral faces has been removed.

Replace, symmetrically, the edges either of the oktahedron or of the cube, and we produce a set of parts of the faces of what is called a Rhombic Dodekahedron (Fig. 41). Many natural crystals assume symmetrical forms containing faces belonging respectively to the oktahedron, the cube truncating its summits, and the rhombic dodekahedron replacing its edges.

Instead of replacing each edge by a single plane equally inclined to the faces which meet in that edge, suppose we *bevel* it symmetrically by two planes. We shall now no longer have a form with fixed angles (*i.e.* invariable shape) as in the cases previously mentioned, but one which (though its general character is determined) will have its form dependent on the inclination of the bevelling planes to the faces which meet in the edge bevelled. Here, again, the geometrical results are found to fit the forms actually presented in nature.

It is an interesting and instructive work for the student to verify these statements by operating on a lump of soft chalk, or (still better) stiff putty or modeller's clay, by means of a broad, but thin, bladed knife.

328. This is not the place to enter into crystallographic details, but we may introduce the following statement, which includes all the above results, and which is found, at all events, to accord fully with the general appearance of any of the very numerous crystals belonging to what is called the cubic system.

Any three axes which meet, but which do not lie in one plane, being chosen, the equation of any plane whatever, which does not pass through the origin, can be written in the form

$$hx + ky + lz = 1.$$

Here h, k, and l are finite quantities, the reciprocals of the distances from the origin at which the plane meets the axes respectively.

To obtain all the plane faces of any one simple form of crystal, all we have to do is to give to h, k, l in the above expression all admissible values in succession.

329. When we take the cubic system, of which alone we have yet spoken, symmetry shows that if the three axes be taken as the lines joining pairs of opposite summits of the oktahedron, obtained as in § 326 above, these will possess properties absolutely alike. Thus symmetry further shows that the numbers h, k, l *may be arranged in any order*, for what is true of any one of the axes is true of each of the others. Similar considerations show that each of h, k, l, independent of the others, *may be either positive or negative.* These quantities are always found, in actual crystals, to have their ratios rigorously expressible in small *Whole Numbers.* [This, of itself, is a very strong argument in favour of the notion that the crystal is built up of little parts, all equal to one another.]

The numbers h, k, l can be arranged in six different orders; and, as any one of these orders has eight possible arrangements of signs, there are forty-eight symmetrically arranged plane triangular faces on the most general simple form of this system. This form is found in many natural crystals, and is called a hexakis-oktahedron (Fig. 37).

n two of h, k, l are equal, it is usual to divide the to two groups according as the third is less or greater than the others. Both classes have twenty-four faces only, but in the first group they are triangular, in the second quadrilateral. These are, respectively, the triakis-oktahedron (Fig. 38) and the eikositetrahedron (Fig. 39) which are derived respectively from the oktahedron by symmetrical bevelling of its edges, and by symmetrical blunting of its summits.

Intermediate to these there is the case when all three of h, k, l are equal, and we have the oktahedron itself.

There is also a limiting case when the third vanishes. This is the rhombic dodekahedron (Fig. 41) discussed in § 327; and of course it also forms the limiting case of the series when one of h, k, l vanishes and the others are unequal, called the tetrakis-hexahedron (Fig. 40), obtained by symmetrical bevelling of the edges of the cube.

When two vanish we have the cube.

The following figure shows the Hexakis-Oktahedron, an octant (lying among the positive axes of x, y, z), being specially lettered for reference, and we can easily see how it degrades into the other forms.

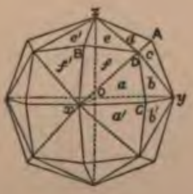

FIG. 37.

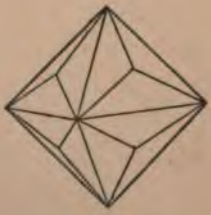

FIG. 38.

Thus if xCy, and therefore yAz and zBx, become straight lines (*i.e.* if the planes marked a and b, c and d, e and f, coincide in pairs), the figure becomes the triakis-oktahedron (Fig. 38). This degrades into the oktahedron if D be in the plane xyz (*i.e.* if a, b, c, d, e, f, are all one plane): and into the rhombic dodekahedron if DC be perpendicular to the plane of xy (*i.e.* if a, b, a', b', are all parts of one plane).

Again, if the planes f and a, b and c, d and e, coincide in pairs, we have the eikositetrahedron (Fig. 39).

If ADB be parallel to the plane of xy, etc., the angle ADB is a right angle, the planes f and f', a and a', b and b', etc., coincide in pairs, and the figure is the tetrakishexahedron (Fig. 40), which becomes the cube when f', f, a, a', etc., are all in one plane; and the rhombic dodekahedron when a, a', b and b', are all in one plane.

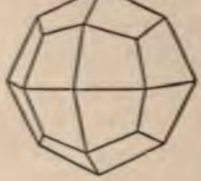

FIG. 39.

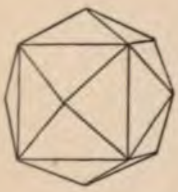

FIG. 40.

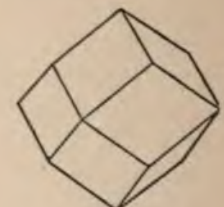

FIG. 41.

330. Six faces of the rhombic dodekahedron are parallel to the line joining D with the origin, and are situated symmetrically round it. If, then, these faces

be extended in their own planes without any alteration of the two groups of three forming the other faces, the whole will become a regular hexagonal prism with ends consisting each of three equal rhombic faces with their greater angles in contact at the summit. This is found, by measurement, to be the form of a bee's cell. And a series of these dodekahedra (all equally and similarly distorted from equal rhombic dodekahedra) can (like them) be so packed together as to fill space without leaving interstices, as in a honeycomb.

331. If, instead of building up a mass of equal spheres, we use similar, equal, and similarly situated ellipsoids of revolution, we must make corresponding alterations in our rules for h, k, l above. If the chief axes of the ellipsoids be perpendicular to the layers of particles, the x, y, z axes are still a rectangular system, but h (say) is no longer interchangeable with k or with l. For h is now a small integral multiple of a parameter which depends on the *chief* axis of the ellipsoid, while k and l are similar multiples of the (equal) parameters of the other two axes. This greatly reduces the number of possible faces in the simple forms.

If the particles are similar, equal, and similarly situated ellipsoids, touching one another in a layer by the ends of two of their axes, the x, y, z system is still rectangular, but no two of h, k, l are interchangeable.

In any other arrangement, which is capable of giving a homogeneous whole, the simplest x, y, z system is no longer rectangular; and the ellipsoids, though still similar, equal, and similarly situated, may come in contact (in threes) in an infinity of different ways.

There is no known form of crystallised matter whose separate faces cannot be exactly accounted for by results based on these premises, though there are many cases of hemihedry, etc., in which the faces geometrically determined for a simple form present themselves only in selected groups.

332. In what we have said above, the only assumptions made (for the purpose of explaining homogeneity) were that the particles grouped were themselves equal, similar, and similarly situated; and that the arrangement of its neighbours round it was exactly the same for each particle.

But it is easy to conceive that very different results may be obtained, even with identical materials, according to circumstances. Every one who has seen water which is prevented only by currents from becoming solid ice, and which is full of excessively small ice crystals, may easily imagine a state of things in which the particles (which, otherwise, would have been deposited one by one to form a crystal) may arrange themselves in very small, but similar and equal, groups before they are deposited. Thus the aggregates, above contemplated, may be built up, not directly of individual particles, but of other less complex though (among themselves) similar and equal sets of aggregated particles. Here again we come back to the same idea as that in the quotation from Clerk-Maxwell (§ 253), and may employ it for the purpose of explaining the existence of cleavage planes, etc.

333. The aggregations we have considered have been such as take place freely; but if we consider what is likely to happen under circumstances of temporary or permanent stress, as, for instance, in a Rupert's drop, or any other melted mass of which a portion is cooled and solidified more suddenly than the rest, we see

that we cannot expect a result in which the potential energy of the whole shall be as small as possible; and are, therefore, prepared to find that such bodies, unless carefully annealed, are essentially in unstable equilibrium.

APPENDIX I. (§ 18).

HYPOTHESES AS TO THE CONSTITUTION OF MATTER.

By Professor Flint, D.D.

1. ALL material substances are infinitely divisible into parts of the same nature as themselves and as complex, even qualitatively, as themselves.

2. All material substances are divisible into ultimate indivisible homogeneous parts as complex as the wholes.

One or other of these two hypotheses (it is, perhaps, impossible to determine which) is attributed by Lucretius to Anaxagoras, whose real opinion, however, was probably the one which follows.

3. All material substances are formed from a primitive matter, "in which all things were together, infinitely numerous, infinitely little," and of which each infinitely little part was infinitely complex.

4. All material substances result from the combination of a few kinds of material elements, each of which is composed of particles like to itself, *e.g.*, earth of earthy particles, water of aqueous particles, air of aerial particles. —This was the hypothesis of the Hindu Kanada, the Greek Empedocles, and a host of medieval *physicists*.

5. All material substances are states or stages of one

primitive matter or element, *e.g.* of water or air.—The hypothesis of Thales, Anaximenes, etc.

6. All material substances are divisible into ultimate indivisible parts, "strong in solid singleness," which have no qualitative but only quantitative differences, and which variously aggregate through motion in a void.

This is the atomic hypothesis as taught by Democritus, Epicurus, etc.

7. All material substances are divisible into elementary substances which are subdivisible into molecules, and, ultimately, into atoms possessed of distinctive qualitative as well as quantitative differences.—Recently, and probably still, the ordinary chemical hypothesis.

8. All material substances are divisible into so-called elementary substances composed of molecular particles of the same nature as themselves, but these molecular particles are complicated structures consisting of congregations of truly elementary atoms, identical in nature and differing only in position, arrangement, motion, etc., and the molecules or chemical atoms are produced from the true or physical atoms by processes of evolution under conditions which Chemistry has not yet been able to reproduce.—Hypothesis of H. Spencer, etc.

9. All material substances are composed of atoms, not hard and solid and on that account indivisible, but the rotatory rings or infinitesimal whirls of an incompressible frictionless fluid, supposed to be homogeneous and perfect, but the nature of which is not otherwise described; and all the differences of material substances are due to the *characters* and *behaviour* of their component rings or whirls.—The hypothesis of Sir William Thomson.

10. The matter which is the object of the senses is the product of a world-building power moulding in accord-

ance with eternal ideas an uncreated substratum, the "receptacle" and "nurse" of "forms," but itself devoid of form and definite attributes.—Plato's hypothesis.

11. The matter which is an object of sense is a synthesis of *form* with a *primary* matter which is merely capacity and passivity—a synthesis produced by a formative cause, which must be both an efficient and final cause.—Aristotle's hypothesis.

12. Impenetrability is the essence of matter.—Hypothesis of various physicists.

13. Extension, not impenetrability, is the essence of matter. "Give me extension and motion and I will construct the world."—Descartes.

14. Material things are "modes" of extension, which is one of the only two discoverable "attributes" of the one "Substance."—Spinoza.

15. Matter in its ultimate constitution consists of *metaphysical points* which give rise to sensible matter by states of effort (*conatus*) transitional from rest to motion. —The hypothesis of Vico. See my "Vico."

16. The ultimate elements of matter are indivisible points without extension, but surrounded by spheres of attractive and repulsive force which alternate according to the distance of these points up to a certain degree of remoteness.—Hypothesis of Boscovich.

17. The physical universe is constituted by the unconscious perceptions of a vast collection of unextended spiritual forces or monads, endowed with a power of spontaneous development and with something of the nature of desire and sentiment: and the properties which physical science ascribes to the ultimate elements of matter are the modes under which the reciprocal actions of the monads appear to sense.—The hypothesis of Leibnitz.

18. Matter is a mental picture in which "mind-stuff" is the thing represented, and mind-stuff is constituted by feelings which can exist by themselves, without forming parts of a consciousness, but which are also woven into the complex form of human minds.—The hypothesis of Clifford.

19. Matter apart from perception has no existence; physical phenomena are essentially *sensations* or *ideas;* "bodies" are groups or clusters of actual or expected sensations arranged according to so-called laws of nature in which is manifest the working of the divine mind.—Berkeley's hypothesis.

20. Matter is simply an appearance to sense, without anything real in it.—The Hindu hypothesis of Maya, the Eleatic hypothesis of non-being, etc.

21. Matter is "the permanent possibility of sensations."—J. S. Mill.

22. "Die Materie ist Dasjenige, wodurch der Wille, der das innere Wesen der Dinge ausmacht, in die Wahrnehmbarkeit tritt, anschaulich, *Sichtbar* wird. In diesem Sinne ist also die Materie die blosse *Sichtbarkeit* des Willens, oder das Band der Welt als Wille mit der Welt als Vorstellung. Die Materie ist durch und durch Causalität."—Schopenhauer.

23. Matter is constituted by forces which are outgoings or manifestations of the Divine Will.

24. Matter is not objectified Will but objectified thought.

25. Matter is Nature's self-externality in its most universal form with a tendency to self-internality or individuation shown in the *nisus* of gravitation, and nature is the Idea in the form of otherness, or alienation.—Hegel.

APPENDIX II. (§§ 29, 101).

From the article "Atom," by Clerk-Maxwell,
Ency. Brit., 9th ed.

ATOM (ἄτομος) is a body which cannot be cut in two. The atomic theory is a theory of the constitution of bodies, which asserts that they are made up of atoms. The opposite theory is that of the homogeneity and continuity of bodies, and asserts, at least in the case of bodies having no apparent organisation, such, for instance, as water, that as we can divide a drop of water into two parts which are each of them drops of water, so we have reason to believe that these smaller drops can be divided again, and the theory goes on to assert that there is nothing in the nature of things to hinder this process of division from being repeated over and over again, times without end. This is the doctrine of the infinite divisibility of bodies, and it is in direct contradiction with the theory of atoms.

The atomists assert that after a certain number of such divisions the parts would be no longer divisible, because each of them would be an atom. The advocates of the continuity of matter assert that the smallest conceivable body has parts, and that whatever has parts may be divided.

. . . . . .

There are thus two modes of thinking about the constitution of bodies, which have had their adherents both in ancient and in modern times. They correspond to the two methods of regarding quantity—the arithmetical and the geometrical. To the atomist the true method of estimating the quantity of matter in a body is to count the atoms in it. The void spaces between the atoms count for nothing. To those who identify matter with extension, the volume of space occupied by a body is the only measure of the quantity of matter in it.

Of the different forms of the atomic theory, that of Boscovich may be taken as an example of the purest monadism. According to Boscovich matter is made up of atoms. Each atom is an indivisible point, having position in space, capable of motion in a continuous path, and possessing a certain mass, whereby a certain amount of force is required to produce a given change of motion. Besides this the atom is endowed with potential force, that is to say, that any two atoms attract or repel each other with a force depending on their distance apart. The law of this force, for all distances greater than say the thousandth of an inch, is an attraction varying as the inverse square of the distance. For smaller distances the force is an attraction for one distance and a repulsion for another, according to some law not yet discovered. Boscovich himself, in order to obviate the possibility of two atoms ever being in the same place, asserts that the ultimate force is a repulsion which increases without limit as the distance diminishes without limit, so that two atoms can never coincide. But this seems an unwarrantable concession to the vulgar opinion that two bodies cannot co-exist in the same place. This opinion is deduced from our experience of the behaviour of bodies of sensible size, but we have no experimental evidence that two atoms may not sometimes coincide. For instance, if oxygen and hydrogen combine to form water, we have no experimental evidence that the molecule of oxygen is not in the very same place with the two molecules of hydrogen. Many persons cannot get rid of the opinion that all matter is extended in length, breadth, and depth. This is a prejudice of the same kind with the last, arising from our experience of bodies consisting of immense multitudes of atoms. The system of atoms, according to Boscovich, occupies a certain region of space in virtue of the forces acting between the component atoms of the system and any other atoms when brought near them. No other system of atoms can occupy the same region of space at the same time, because, before it could do so, the mutual action of the atoms would have caused a repulsion between the two systems insuperable by any force which we can command. Thus, a number of soldiers with firearms may occupy an extensive region to the exclusion of the enemy's armies, though the space filled by their bodies is but small. In this way Boscovich explained the apparent extension of bodies consisting of atoms, each of which is devoid of extension. According to Boscovich's theory, all action between bodies is action at a distance. There is no such thing in nature as actual contact between two bodies. When two bodies are said in ordinary language to be in contact, all that is meant is that they are so near together that the repulsion between the nearest pairs of atoms belonging to the two bodies is very great.

Thus, in Boscovich's theory, the atom has continuity of existence in time and space. At any instant of time it is at some point of space, and it is never in more than one place at a time. It passes from one place to another along a continuous path. It has a definite mass which cannot be increased or diminished. Atoms are endowed with the power of acting on one another by attraction or repulsion, the amount of the force depending on the distance between them. On the other hand, the atom itself has no parts or dimensions. In its geometrical aspect it is a mere geometrical point. It has no extension in space. It has not the so-called property of Impenetrability, for two atoms may exist in the same place. This we may regard as one extreme of the various opinions about the constitution of bodies.

The opposite extreme, that of Anaxagoras—the theory that bodies apparently homogeneous and continuous are so in reality—is, in its extreme form, a theory incapable of development. To explain the properties of any substance by this theory is impossible. We can only admit the observed properties of such substance as ultimate facts. There is a certain stage, however, of scientific progress in which a method corresponding to this theory is of service. In hydrostatics, for instance, we define a fluid by means of one of its known properties, and from this definition we make the system of deductions which constitutes the science of hydrostatics. In this way the science of hydrostatics may be built upon an experimental basis, without any consideration of the constitution of a fluid as to whether it is molecular or continuous. In like manner, after the French mathematicians had attempted, with more or less ingenuity, to construct a theory of elastic solids from the hypothesis that they consist of atoms in equilibrium under the action of their mutual forces, Stokes and others showed that all the results of this hypothesis, so far at least as they agreed with facts, might be deduced from the postulate that elastic bodies exist, and from the hypothesis that the smallest portions into which we can divide them are sensibly homogeneous. In this way the principle of continuity, which is the basis of the method of Fluxions and the whole of modern mathematics, may be applied to the analysis of problems connected with material bodies by assuming them, for the purpose of this analysis, to be homogeneous. All that is required to make the results applicable to the real case is that the smallest portions of the substance of which we take any notice shall be sensibly of the same kind. Thus, if a railway contractor has to make a tunnel through a hill of gravel, and if one cubic yard of the gravel is so like another cubic yard that for the purposes of the contract they may be taken as equivalent, then, in estimating the work required to remove the gravel from the tunnel, he may, without fear of error, make his calculations as if the gravel were a continuous substance. But if a worm has to make his way through the gravel, it makes the greatest possible difference to him whether he tries to push right against a piece of gravel, or directs his course through one of the intervals between the pieces; to him, therefore, the gravel is by no means a homogeneous and continuous substance.

In the same way, a theory that some particular substance, say water, is homogeneous and continuous may be a good working theory up to a certain point, but may fail when we come to deal with quantities so minute or so attenuated that their heterogeneity of structure comes into prominence. Whether this heterogeneity of structure is or is not consistent with homogeneity and continuity of substance is another question.

The extreme form of the doctrine of continuity is that stated by Descartes, who maintains that the whole universe is equally full of matter, and that this matter is all of one kind, having no essential property besides that of extension. All the properties which we perceive in matter he reduces to its parts being movable among one another, and so capable of all the varieties which we can perceive to follow from the motion of its parts (*Principia*, ii. 23). Descartes' own attempts to deduce the different qualities and actions of bodies in this way are not of much value. More than a century was required to invent methods of investigating the conditions of the motion of systems of bodies such as Descartes imagined.

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A cube, whose side is the 4000th of a millimètre, may be taken as the *minimum visibile* for observers of the present day. Such a cube would contain from 60 to 100 million molecules of oxygen or of nitrogen; but since the molecules of organised substances contain on an average about 50 of the more elementary atoms, we may assume that the smallest organised particle visible under the microscope contains about two million molecules of organic matter. At least half of every living organism consists of water, so that the smallest living being visible under the microscope does not contain more than about a million organic molecules. Some exceedingly simple organism may be supposed built up of not more than a million similar molecules. It is impossible, however, to conceive so small a number sufficient to form a being furnished with a whole system of specialised organs.

Thus molecular science sets us face to face with physiological theories. It forbids the physiologist from imagining that structural details of infinitely small dimensions can furnish an explanation of the infinite variety which exists in the properties and functions of the most minute organisms.

A microscopic germ is, we know, capable of development into a highly organised animal. Another germ, equally microscopic, becomes, when developed, an animal of a totally different kind. Do all the differences, infinite in number, which distinguish the one animal from the other, arise each from some difference in the structure of the respective germs? Even if we admit this as possible, we shall be called upon by the advocates of Pangenesis to admit still greater marvels. For the microscopic germ, according to this theory, is no mere individual, but a representative body, containing members collected from every rank of the long-drawn ramification of the ancestral tree, the number of these members being amply sufficient not only to furnish the hereditary characteristics of every organ of the body and every habit of the animal from birth to death, but also to afford a stock of latent gemmules to be passed on in an inactive state from germ to germ, till at last the ancestral peculiarity which it represents is revived in some remote descendant.

Some of the exponents of this theory of heredity have attempted to elude the difficulty of placing a whole world of wonders within a body so small and so devoid of visible structure as a germ, by using the phrase structureless germs.[1] Now, one material system can differ from another only in the configuration and motion which it has at a given instant. To explain differences of function and development of a germ without assuming differences of structure is, therefore, to admit that the properties of a germ are not those of a purely material system.

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[1] See F. Galton, "On Blood Relationship," *Proc. Roy. Soc.*, June 13, 1872.

Coincidences observed, in the case of several terrestrial substances, with several systems of lines in the spectra of the heavenly bodies, tend to increase the evidence for the doctrine that terrestrial substances exist in the heavenly bodies, while the discovery of particular lines in a celestial spectrum which do not coincide with any line in a terrestrial spectrum does not much weaken the general argument, but rather indicates either that a substance exists in the heavenly body not yet detected by chemists on earth, or that the temperature of the heavenly body is such that some substance, undecomposable by our methods, is there split up into components unknown to us in their separate state.

We are thus led to believe that in widely-separated parts of the visible universe molecules exist of various kinds, the molecules of each kind having their various periods of vibration either identical, or so nearly identical that our spectroscopes cannot distinguish them. We might argue from this that these molecules are alike in all other respects, as, for instance, in mass. But it is sufficient for our present purpose to observe that the same kind of molecule, say that of hydrogen, has the same set of periods of vibration, whether we procure the hydrogen from water, from coal, or from meteoric iron, and that light, having the same set of periods of vibration, comes to us from the sun, from Sirius, and from Arcturus.

The same kind of reasoning which led us to believe that hydrogen exists in the sun and stars, also leads us to believe that the molecules of hydrogen in all these bodies had a common origin. For a material system capable of vibration may have for its periods of vibration any set of values whatever. The probability, therefore,

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the molecules which did not fall under one of the very limited number of kinds known to us; and to get rid of a number of indestructible bodies, exceeding by far the number of the molecules of all the recognised kinds, would be one of the severest labours ever proposed to a cosmogonist.

It is well known that living beings may be grouped into a certain number of species, defined with more or less precision, and that it is difficult or impossible to find a series of individuals forming the links of a continuous chain between one species and another. In the case of living beings, however, the generation of individuals is always going on, each individual differing more or less from its parents. Each individual during its whole life is undergoing modification, and it either survives and propagates its species, or dies early, accordingly as it is more or less adapted to the circumstances of its environment. Hence, it has been found possible to frame a theory of the distribution of organisms into species by means of generation, variation, and discriminative destruction. But a theory of evolution of this kind cannot be applied to the case of molecules, for the individual molecules neither are born nor die, they have neither parents nor offspring, and so far from being modified by their environment, we find that two molecules of the same kind, say of hydrogen, have the same properties, though one has been compounded with carbon and buried in the earth as coal for untold ages, while the other has been "occluded" in the iron of a meteorite, and after unknown wanderings in the heavens has at last fallen into the hands of some terrestrial chemist.

The process by which the molecules become distributed into distinct species is not one of which we know any instances going on at present, or of which we have as yet been able to form any mental representation. If we suppose that the molecules known to us are built up each of some moderate number of atoms, these atoms being all of them exactly alike, then we may attribute the limited number of molecular species to the limited number of ways in which the primitive atoms may be combined so as to form a permanent system.

But though this hypothesis gets rid of the difficulty of accounting for the independent origin of different species of molecules, it merely transfers the difficulty from the known molecules to the primitive atoms. How did the atoms come to be all alike in those properties which are in themselves capable of assuming any value?

If we adopt the theory of Boscovich, and assert that the primitive atom is a mere centre of force, having a certain definite mass, we may get over the difficulty about the equality of the mass of all atoms by laying it down as a doctrine which cannot be disproved by experiment, that mass is not a quantity capable of continuous increase or diminution, but that it is in its own nature discontinuous, like number, the atom being the unit, and all masses being multiples of that unit. We have no evidence that it is possible for the ratio of two masses to be an incommensurable quantity, for the incommensurable quantities in geometry are supposed to be traced out in a continuous medium. If matter is atomic, and therefore discontinuous, it is unfitted for the construction of perfect geometrical models, but in other respects it may fulfil its functions.

But even if we adopt a theory which makes the equality of the mass of different atoms a result depending on the nature of mass rather than on any quantitative adjustment, the correspondence of the periods of vibration of actual molecules is a fact of a different order.

We know that radiations exist having periods of vibration of every value between those corresponding to the limits of the visible spectrum, and probably far beyond these limits on both sides. The most powerful spectroscope can detect no gap or discontinuity in the spectrum of the light emitted by incandescent lime.

The period of vibration of a luminous particle is therefore a quantity which in itself is capable of assuming any one of a series of values, which, if not mathematically continuous, is such that consecutive observed values differ from each other by less than the ten-thousandth part of either. There is, therefore, nothing in the nature of time itself to prevent the period of vibration of a molecule from assuming any one of many thousand different observable values. That which determines the period of any particular kind of vibration is the relation which subsists between the corresponding type of displacement and the force of restitution thereby called into play, a relation involving constants of space and time as well as of mass.

It is the equality of these space- and time-constants for all molecules of the same kind which we have next to consider. We have seen that the very different circumstances in which different molecules of the same kind have been placed have not, even in the course of many ages, produced any appreciable difference in the values of these constants. If, then, the various processes of nature to which these molecules have been subjected since the world began have not been able in all that time to produce any appreciable difference between the constants of one molecule and those of another, we are forced to conclude that it is not to the operation of any of these processes that the uniformity of the constants is due.

The formation of the molecule is therefore an event not belonging to that order of nature under which we live. It is an operation of a kind which is not, so far as we are aware, going on on earth or in the sun or the stars, either now or since these bodies began to be formed. It must be referred to the epoch, not of the formation of the earth or of the solar system, but of the establishment of the existing order of nature, and till not only these worlds and systems, but the very order of nature itself is dissolved, we have no reason to expect the occurrence of any operation of a similar kind.

In the present state of science, therefore, we have strong reasons for believing that in a molecule, or if not in a molecule, in one of its component atoms, we have something which has existed either from eternity or at least from times anterior to the existing order of nature. But besides this atom, there are immense numbers of other atoms of the same kind, and the constants of each of these atoms are incapable of adjustment by any process now in action. Each is physically independent of all the others.

Whether or not the conception of a multitude of beings existing from all eternity is in itself self-contradictory, the conception becomes palpably absurd when we attribute a relation of quantitative equality to all these beings. We are then forced to look beyond them to some common cause or common origin to explain why this singular relation of equality exists, rather than any one of the infinite number of possible relations of inequality.

Science is incompetent to reason upon the creation of matter itself out of nothing. We have reached the utmost limit of our thinking faculties when we have admitted that, because matter cannot be eternal and self-existent, it must have been created. It is only when we contemplate not matter in itself, but the form in which it actually exists, that our mind finds something on which it can lay hold.

That matter, as such, should have certain fundamental properties, that it should have a continuous existence in space and time, that all action should be between two portions of matter, and so on, are truths which may, for aught we know, be of the kind which metaphysicians call necessary. We may use our knowledge of such truths for purposes of deduction, but we have no data for speculating on their origin.

But the equality of the constants of the molecules is a fact of a very different order. It arises from a particular distribution of matter, a *collocation*, to use the expression of Dr. Chalmers, of things which we have no difficulty in imagining to have been arranged otherwise. But many of the ordinary instances of collocation are adjustments of constants, which are not only arbitrary in their own nature, but in which variations actually occur; and when it is pointed out that these adjustments are beneficial to living beings, and are therefore instances of benevolent design, it is replied that those variations which are not conducive to the growth and multiplication of living beings tend to their destruction, and to the removal thereby of the evidence of any adjustment not beneficial.

The constitution of an atom, however, is such as to render it, so far as we can judge, independent of all the dangers arising from the struggle for existence. Plausible reasons may, no doubt, be assigned for believing that if the constants had varied from atom to atom through any sensible range, the bodies formed by aggregates of such atoms would not have been so well fitted for the construction of the world as the bodies which actually exist. But as we have no experience of bodies formed of such variable atoms this must remain a bare conjecture.

Atoms have been compared by Sir J. Herschel to manufactured articles, on account of their uniformity. The uniformity of manufactured articles may be traced to very different motives on the part of the manufacturer. In certain cases it is found to be less expensive as regards trouble, as well as cost, to make a great many objects exactly alike than to adapt each to its special requirements. Thus, shoes for soldiers are made in large numbers without any designed adaptation to the feet of particular men. In another class of cases the uniformity is intentional, and is designed to make the manufactured article more valuable. Thus, Whitworth's bolts are made in a certain number of sizes, so that if one bolt is lost, another may be got at once, and accurately fitted to its place. The identity of the arrangement of the words in the different copies of a document or book is a matter of great practical importance, and it is more perfectly secured by the process of printing than by that of manuscript copying.

In a third class not a part only but the whole of the value of the object arises from its exact conformity to a given standard. Weights and measures belong to this class, and the existence of many well-adjusted material standards of weight and measure in any country furnishes evidence of the existence of a system of law regulating the transactions of the inhabitants, and enjoining in all professed measures a conformity to the national standard.

There are thus three kinds of usefulness in manufactured articles—cheapness, serviceableness, and quantitative accuracy. Which of these was present to the mind of Sir J. Herschel we cannot now positively affirm, but it was at least as likely to have been the last as the first, though it seems more probable that he meant to assert that a number of exactly similar things cannot be each of them eternal and self-existent, and must therefore have been made, and that he used the phrase "manufactured article" to suggest the idea of their being made in great numbers.

APPENDIX III. (§ 97).

TUM vero ex eo inventionis ingressu duas dicitur fecisse massas æquo pondere, quo etiam fuerat corona, unam ex auro alteram ex argento. Cum ita fecisset, vas amplum ad summa labra implevit aqua; in quo demisit argenteam massam: cuius quanta magnitudo in vase depressa est, tantum aquæ effluxit. Ita exempta massa, quanto minus factum fuerat, refudit sextario mensus, ut eodem modo, quo prius fuerat, ad labra æquaretur. Ita ex eo invenit, quantum [*ad certum*] pondus argenti ad certam aquæ mensuram responderet. Cum id expertus esset, tum auream massam similiter pleno vase demisit, et ea exempta, eadem ratione mensura addita invenit ex aqua non tantum defluxisse sed [*tantum*] minus, quanto minus magno corpore eodem pondere auri massa esset quam argenti. Postea vero repleto vase in eadem aqua ipsa corona demissa, invenit plus aquæ defluxisse in coronam, quam in auream eodem pondere massam: et ita ex eo, quod plus defluxerat aquæ in corona quam in massa, ratiocinatus deprehendit argenti in auro mixtionem et manifestum furtum redemptoris.—Vitruvius, *De Architecturâ*, Lib. IX., *Præfatio.*

APPENDIX IV. (§ 191).

NOTE ON A SINGULAR PASSAGE IN THE "PRINCIPIA."

By Professor Tait.[1]

IN the remarkable *Scholium*, appended to his chapter on the Laws of Motion, where Newton is showing what Wren, Wallis, and Huygens had done in connection with the impact of bodies, he uses the following very peculiar language:—

"Sed et veritas comprobata est a *D. Wrenno* coram *Regiâ Societate* per experimentum Pendulorum, quod etiam *Clarissimus Mariottus* Libro integro exponere mox dignatus est."

The last clause of this sentence, which I had occasion to consult a few days ago, appeared to me to be so sarcastic, and so unlike in tone to all the context, that I was anxious to discover its full intention.

Not one of the Commentators, to whose works I had access, makes any remark on the passage. The Translators differ widely.

Thus Motte softens the clause down into the trivial remark "which Mr Mariotte soon after thought fit to explain in a treatise entirely on that subject."

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[1] *Proc. R.S.E.*, January 19, 1885.

The Marquise du Chastellet (1756) renders it thus:—
". . . . mais ce fut *Wrenn* qui les confirma par des Expériences faites avec des Pendules devant la Société Royale: lesquelles le célèbre *Mariotte* a rapportées depuis dans un Traité qu'il a composé exprès sur cette matière."

Thorp's translation (1777) runs:—
"which the very eminent Mr Mariotte soon after thought fit to explain in a treatise entirely upon that subject."

Finally, Wolfers (1872) renders it thus:—
"der zweite zeigte der Societät die Richtigkeit seiner Erfindung an einem Pendelversuche, den der berühmte Mariotte in seinem eigenen Werke aus einander zu setzen, für würdig erachtete."

Not one of these seems to have remarked anything singular in the language employed. But when we consult the "entire book" in which Mariotte is said by Newton to have "expounded" the result of Wren, and which is entitled *Traité de la Percussion ou Choc des Corps*, we find that the name of Wren is not once mentioned in its pages! From the beginning to the end there is nothing calculated even to hint to the reader that the treatise is not wholly original.

This gives a clue to the reason for Newton's sarcastic language; whose intensity is heightened by the contrast between the *Clarissimus* which is carefully prefixed to the name of Mariotte, and the simple *D.* prefixed, not only to the names of Englishmen like Wren and Wallis, but even to that of a specially distinguished foreigner like Huygens.

Newton must, of course, like all the scientific men of the time (Mariotte included), have been fully cognisant of Boyle's celebrated controversy with Linus, which led to the publication, in 1662, of the *Defence of the Doctrine touching the Spring and Weight of the Air*. In that tract, Part II. Chap. v., the result called in Britain *Boyle's Law* is established (by a very remarkable series of experiments) for pressures less than, as well as for pressures greater than, an atmosphere; and it is established by means of the very form of apparatus still employed for the purpose in lecture demonstrations. Boyle, at least, claimed originality, for he says in connection with the difficulties met with in the breaking of his glass tube,—". . . . an accurate Experiment of this nature would be of great importance to the Doctrine of the Spring of the Air, and has not been made (that I know) by any man. . . ."

In Mariotte's *Discours de la Nature de l'Air*, published FOURTEEN years later than this work of Boyle, we find no mention whatever of Boyle, though the identical form of apparatus used by Boyle is described. The whole work proceeds, as does that on *Percussion*, with a calm ignoration of the labours of the majority of contemporary philosophers.

This also must, of course, have been perfectly well known to Newton:—and we can now see full reason for the markedly peculiar language which he permits himself to employ with reference to Mariotte.

What was thought of this matter by a very distinguished foreign contemporary, appears from the treatise of James Bernoulli, *De Gravitate Ætheris*, Amsterdam, 1683, p. 92.

"Veritas utriusque hujus regulæ manifesta fit duobus curiosis experimentis, ab Illustr. Dn. Boylio hanc in rem factis, quæ videsis in *Tractatu ejus contrà Linum*, *Cap.* V., cui duas Auctor subjunxit Tabulas pro diversis Condensationis et Rarefactionis gradibus."

In order to satisfy myself that Newton's language, taken in its obvious meaning, really has the intention which I could not avoid attaching to it, I requested my colleague Professor Butcher to state the impression which it produced on him. I copied for him the passage above quoted, putting A. for the word *Wrenno*, and B. for *Mariottus*; and I expressly avoided stating who was the writer. Here is his reply:—

"I imagine the point of the passage to be something of this kind (speaking without farther context or acquaintance with the Latinity of the learned author):—

"A established the truth by means of a (simple) experiment, before the Royal Society; later, B thought it worth his while to write a whole book to prove the same point.

"I should take the tone to be highly sarcastic at B's expense. It *seems* to suggest that B was not only clumsy but dishonest. The latter inference is not certain, but at any rate we have a *hint* that B took no notice of A's discovery, and spent a deal of useless labour."

This conclusion, it will be seen, agrees exactly with the complete ignoration of Wren by Mariotte.

When I afterwards referred Professor Butcher to the whole context, in my copy of the first edition of the *Principia*, and asked him whether the use of *Clarissimus* was sarcastic or not, he wrote—

"I certainly think so. Indeed, even apart from the context, I thought the *Clarissimus* was ironical, but there can be no doubt of it when it corresponds to *D.* Wren."

In explanation of this I must mention that, when I first sent the passage to Professor Butcher, I had copied it from Horsley's sumptuous edition; in which the *Ds* are omitted, while the *Clarissimus* is retained.

Alike in France and in Germany, to this day, the Law in question goes by the name of Mariotte. The following extracts, from two of the most recent high-class text-books, have now a peculiar interest. I have put a word or two of each in italics. These should be compared with the dates given.

"Diese Frage ist schon frühzeitig untersucht und zwar *fast gleichzeitig* von dem französischen Physiker Mariotte (1679) und dem englischen Physiker Boyle (1662)." Wüllner, *Lehrbuch der Experimentalphysik* 1882, § 98.

"La loi qui régit la compressibilité des gaz à température constante a été trouvée *presque simultanément* par Boyle (1662) en Angleterre et par Mariotte (1676) en France; toutefois, si Boyle a publié le premier ses expériences, il ne sut pas en tirer l'énoncé clair que donna le physicien français. C'est donc avec quelque raison que le nom de loi de Mariotte a passé dans l'usage." Violle, *Cours de Physique*, 1884, § 283.[1]

On this I need make no remark further than quoting one sentence from Boyle, where he compares the actual pressure, employed in producing a certain compression in air, with "what the pressure should be according to the *Hypothesis*, that supposes the pressures and expansions to be in reciprocal proportion." M. Violle has probably been misled by the archaic use of "expansion" for volume.

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[1] Even in the latest edition of Jamin's *Cours de Physique* we find the statement:—"Les expériences de Boyle se rapportent seulement aux pressions supérieures à la pression atmosphérique." Compare this with Boyle's own words, in § 195 above.

It must be said, in justice to Mariotte, that he does not appear to have *claimed* the discovery of any new facts in connection either with collision or with the effect of pressure on air. He rather appears to write with the conscious infallibility of a man for whom nature has no secrets. And he transcribes, or adapts, into his writings (without any attempt at acknowledgment) whatever suits him in those of other people. He seems to have been a splendidly successful and very early example of the highest class of what we now call the *Paper-Scientists.* Witness the following extracts from Boyle, with a parallel citation from Mariotte of *fourteen* years later date *at least.* The comparison of the sponges had struck me so much, in Mariotte's work, that I was induced to search for it in Boyle, where I felt convinced that I should find it.

"This Notion may perhaps be somewhat further explain'd, by conceiveing the Air near the Earth to be such a heap of little Bodies, lying one upon another, as may be resembled to a Fleece of Wooll. For this (to omit other likenesses betwixt them) consists of many slender and flexible Hairs; each of which, may indeed, like a little Spring, be easily bent or rouled up; but will also, like a Spring, be still endeavouring to stretch itself out again. For though both these Haires, and the Æreal Corpuscles to which we liken them, do easily yield to externall pressures; yet each of them (by virtue of its structure) is endow'd with a Power or Principle of Selfe-Dilatation; by virtue whereof, though the hairs may by a Mans hand be bent and crouded closer together, and into a narrower room then suits best with the Nature of the Body, yet, whils't the compression lasts, there is in the fleece they composeth an endeavour outwards, whereby it continually thrusts against the hand that opposeth its Expansion. And upon the removall of the externall pressure, by opening the hand more or less, the compressed Wooll doth, as it were, spontaneously expand or display it self towards the recovery of its former more loose and free condition till the Fleece hath either regain'd its former Dimensions, or at least, approached them as neare as the compressing hand, (perchance not quite open'd) will permit. The power of Selfe-Dilatation is somewhat more conspicuous in a dry Spunge compress'd, then in a Fleece of Wooll. But yet we rather chose to employ the latter, on this occasion, because it is not like a Spunge, an intire Body; but a number of slender and flexible Bodies, loosely complicated, as the Air itself seems to be."

And, a few pages later, he adds :—

". . . . a Column of Air, of many miles in height, leaning upon some springy Corpuscles of Air here below, may have weight enough to bend their little springs, and keep them bent: As, (to resume our former comparison,) if there were fleeces of Wooll pil'd up to a mountainous height, upon one another, the hairs that compose the lowermost Locks which support the rest, would, by the weight of all the Wool above them, be as well strongly compress'd as if a Man should squeeze them together in his hands, or employ any such other moderate force to compress them. So that we need not wonder, that upon the taking of the incumbent Air from any parcel of the Atmosphere here below, the Corpuscles, whereof that undermost Air consists, should display themselves, and take up more room than before."

Mariotte (p. 151). "On peut comprendre à peu près cette différence de condensation de l'Air, par l'exemple de plusieurs éponges qu'on auroit entassées les unes sur les autres. Car il est évident, que celles qui seroient tout au haut, auroient leur étenduë naturelle : que celles qui seroient immêdiatement au dessous, seroient un peu moins dilatées ; et que celles qui seroient au dessous de toutes les autres, seroient trés-serrées et condensées. Il est encore manifeste, que si on ôtoit toutes celles du dessus, celles du dessous reprendroient leur étenduë naturelle par la vertu de ressort qu'elles ont, et que si on en ôtoit seulement une partie, elles ne reprendroient qu'une partie de leur dilatation."

Those curious in such antiquarian details will probably find a rich reward by making a careful comparison of these two works; and in tracing the connection between the *Liber integer*, and its fons et origo, the paper of Sir Christopher Wren.

Condorcet, in his *Eloge de Mariotte*, says :—"Les lois du choc des corps avaient été trouvées par une métaphysique et par une application d'analyse, nouvelles l'une et l'autre, et si subtiles, que les démonstrations de ces lois ne pouvaient satisfaire que les grands mathématiciens. Mariotte chercha à les rendre, pour ainsi dire, populaires, en les appuyant sur des expériences, etc." *i.e. precisely* what Wren had thoroughly done before him.

"Le discours de Mariotte sur la nature de l'air renferme encore une suite d'expériences intéressantes, et qui étaient absolument neuves." This, as we have seen, is entirely incorrect.

But Condorcet shows an easy way out of all questions of this kind, however delicate, in the words :—"On ne doit aux morts que ce qui peut être utile aux vivants, la vérité et la justice. Cependant, lorsqu'il reste encore des amis et des enfants que la vérité peut affliger, les

spread delusion:—and th
he can scarcely be said
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La loi de Mariotte (other
as certainly had "greatness

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